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INFLUENCE OF BARRIER DIMENSIONS ON THE VIBRATIONAL SPECTRUM OF ETHANE

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ABSTRACT. Recent investigations on the internal rotation in ethane indicate the existence of a high potential barrier. A sinusoidal shape of barrier is assumed, and to obtain agreement with thermal measurements a height of 3000 cal./mole is required. In this paper the effect of a potential barrier of rectangular shape on the energy levels is investigated. In this case a barrier of height $V=1700$ cal./mole and width 1·35 radians leads to substantially similar energy levels to those predicted by the sinusoidal barrier previously considered. The effect of splitting in the energy levels is found to be slight for levels below V, and considerable for those above V. The energy levels for heavy ethane are also deduced.

It is concluded that the value previously deduced for the barrier height V, i.e. 3000 cal./mole, is only significant for a cosine form of variation. The evidence for this shape is by no means decisive.

§1. INTRODUCTION

RECENT investigations provide evidence for the existence of a potential which restricts rotation about the single C—C bond. Ethane has been chosen as a typical example, and the evidence arises mainly from thermal and spectroscopic results. Calculations based on the vibrational frequencies of the molecule lead to a variation in the specific heat with temperature, which may be compared with direct measurement.

The ethane molecule has eighteen modes of vibration, of which six are non-degenerate and twelve doubly-degenerate. Among recent assignments, that due to Crawford, Avery and Linnett (1938), and based on both infra-red and Raman spectra, is perhaps the most reliable. Of these vibrations, the twisting frequency is spectroscopically inactive, while the degenerate E'' or ${}^2\nu_\alpha{}^M$ frequency is ambiguous. Quite apart from zero-energy considerations, a full calculation is therefore impossible. An alternative procedure is therefore adopted, the uncertain frequency E'' being chosen to obtain agreement with the specific heat at room temperature. If free rotation about the C—C bond is assumed, a frequency of 750 cm^{-1} is required, while with highly restricted rotation a value of 1100 cm^{-1} is found. To distinguish between these two possibilities, the specific heat at low temperatures was investigated (Kistiakowsky and collaborators, 1935 to 1939: Hunsmann, 1938). The results obtained favoured the assumption of a high restricting barrier.

An alternative method of approach makes use of the heat of hydrogenation of ethylene, of which the most accurate determination is that of Kistiakowsky, Romeyn, Ruhoff, Smith and Vaughan (1935). This value may be used in

conjunction with the known modes of vibration to calculate the equilibrium constant K_p in the reaction

$$C_2H_4 + H_2 \rightleftharpoons C_2H_6.$$

With this estimate, Teller and Topley (1935), and also Smith and Vaughan (1935), showed that no agreement could be obtained on the assumption of free rotation. Kemp and Pitzer (1936, 1937), however, showed that, with a high restricting barrier, agreement could be established, the value chosen (3000 cal./mole approximately) also accounting for the observed specific heat and entropy. Guggenheim (1941) verified that the agreement thus obtained between calculated and observed values of K_p was maintained over a wide range of temperature.

The cause of the restricting potential is by no means clear. Eyring (1932) estimated the interaction between electron pairs, and found a potential height of about 300 cal./mole. Penney (1934), using the method of molecular orbitals, concluded that at room temperatures, rotation about the C—C bond is by no means free.

A quantum mechanical investigation of the double rotator with one degree of rotational freedom was made by Nielsen (1932), and subsequently by Teller and Weigert (1933). The potential barrier assumed was a sinusoidal function of the angle between the methyl groups. The equation to be solved is of the form of Mathieu's equation:

$$\frac{d^2M}{dx^2} + (a + 16q\cos 2x)M = 0.$$

Kemp and Pitzer showed that, with a barrier of height 3150 cal./mole, agreement is obtained both with regard to specific-heat measurements and to the equilibrium constant. The shape of barrier assumed was criticized by Kistiakowsky and Wilson (1938) as being of arbitrary character. Pitzer and Kemp (1938) answered this objection, but failed to justify the form of barrier chosen.

Kistiakowsky, Lacher and Stitt (1939), from a study of the specific heat of ethane and heavy ethane, concluded that the existence of energy levels 0, 275 ± 10 cm.$^{-1}$, 520 ± 10 cm.$^{-1}$, and (very approximately) 725 cm.$^{-1}$ would account satisfactorily for the observed specific heat. These levels are equal to those deduced from a cosine form of barrier, of height 2750 cal./mole approximately. It was therefore concluded that these measurements afforded strong evidence for the shape of potential chosen, or at least for its degree of anharmonicity.

In this paper an attempt is made to discover whether different shapes of potential barrier cannot account equally well for the observed levels, which summarize the experimental conclusions. A rectangular type of barrier is chosen, and both height and width are varied. Satisfactory agreement can be obtained with a much lower level, and the evidence for a sinusoidal shape, or even for a corresponding anharmonicity, is therefore by no means decisive.

§2. DETERMINATION OF THE ENERGY LEVELS

The energy levels of the ethane molecule may be determined from the equation

$$\frac{\partial^2\Psi}{\partial\phi_1^2} + \frac{\partial^2\Psi}{\partial\phi_2^2} + \frac{8\pi^2 I}{h^2}[W_T - V]\Psi = 0. \qquad \ldots\ldots(1)$$

Here I is the moment of inertia of each methyl group, and ϕ_1, ϕ_2 are the corresponding angular co-ordinates about the common axis. V is the interaction energy between the end groups, and depends only on the angle $\phi_1-\phi_2$ between them. W_T is the energy of the molecule, and the other symbols have their customary meanings.

To separate the solid-body rotation and torsional motion of the molecule, substitute

$$\xi=\frac{\phi_1+\phi_2}{2}, \quad \eta=\phi_1-\phi_2. \qquad \ldots\ldots(2)$$

Then (1) becomes

$$\frac{1}{2}\frac{\partial^2\Psi}{\partial\xi^2}+2\frac{\partial^2\Psi}{\partial\eta^2}+\frac{8\pi^2I}{h^2}\{W_T-V(\eta)\}\Psi=0.$$

The variables ξ, η are separable.

Let $\Psi=\psi(\xi)\chi(\eta)$, so that

$$\frac{1}{2}\chi\frac{\partial^2\psi}{\partial\xi^2}+2\psi\frac{\partial^2\chi}{\partial\eta^2}+\frac{8\pi^2I}{h^2}\{W_T-V\}\chi\psi=0,$$

$$\frac{1}{2\psi}\frac{\partial^2\psi}{\partial\xi^2}=-\frac{2}{\chi}\frac{\partial^2\chi}{\partial\eta^2}-\frac{8\pi^2I}{h^2}\{W_T-V\}=c, \text{ say,}$$

where c does not depend on χ, η.

The equation for ψ has a general solution:

$$\psi=Ae^{i\Lambda\xi}+Be^{-i\Lambda\xi}, \qquad \ldots\ldots(3)$$

where $\Lambda^2=-2c>0$.

This equation defines the rotational spectrum of the molecule, rotating bodily; this motion is quantized by the integer Λ.

The corresponding equation to determine the twisting energy levels can be written

$$\frac{\partial^2\chi}{\partial\eta^2}+\frac{4\pi^2I}{h^2}\left(W_T-V-\frac{\Lambda^2h^2}{16\pi^2I}\right)\chi=0, \qquad \ldots\ldots(4)$$

or

$$\frac{\partial^2\chi}{\partial\eta^2}+\frac{4\pi^2I}{h^2}(W-V)\chi=0. \qquad \ldots\ldots(4\,a)$$

The quantity $W\left(=W_T-\frac{\Lambda^2h^2}{16\pi^2I}\right)$ represents the energy due to the torsional motion alone, while $\frac{\Lambda^2h^2}{16\pi^2I}$, the remaining part of the energy, is due to solid-body rotation.

Before proceeding to the solution of the equation for the internal twisting energy W, a boundary condition may be stressed. The wave function Ψ must not change if one methyl group is rotated by 2π, since the molecule does not thereby change, i.e.:

$$\Psi(\phi_1,\ \phi_2+2\pi)=\Psi(\phi_1,\ \phi_2).$$

However, if ϕ_2 is increased by 2π, η decreases by 2π, but ξ is only increased by π, so that

$$\psi(\xi)\chi(\eta)=\Psi(\phi_1,\ \phi_2)=\Psi(\phi_1,\ \phi_2+2\pi)=\psi(\xi+\pi)\chi(\eta-2\pi).$$

But

$$\psi(\xi+\pi)=\psi(\xi)e^{i\Lambda\pi}.$$

Hence $$\chi(\eta-2\pi) = \chi(\eta)e^{-i\Lambda\pi} = \pm\chi(\eta), \qquad \ldots\ldots(5)$$

depending on whether Λ is even or odd.

The solutions of the equation

$$\frac{\partial^2\chi}{\partial\eta^2} + K^2(W-V)\chi = 0, \quad \text{where } K^2 = \frac{4\pi^2 I}{h^2}, \qquad \ldots\ldots(6)$$

therefore fall into two classes:

$$\chi(\eta) = \chi(\eta+2\pi); \qquad (\Lambda \text{ even}) \qquad \ldots\ldots(5\,a)$$

$$\chi(\eta) = -\chi(\eta+2\pi). \qquad (\Lambda \text{ odd}) \qquad \ldots\ldots(5\,b)$$

For simplicity, we put $\beta^2 = K^2W$ and $\gamma^2 = K^2(V-W)$, $\ldots\ldots(6a)$ and $(6\,b)$ assuming that $V > W$. The case where $V < W$ will be discussed later.

The potential energy V of interaction between the methyl groups is a function of the angle η between them. The shape of the barrier preventing free rotation about the C—C axis is unknown, and even its cause is not clear. It is, therefore, necessary to choose V as a function of η consistent with the known symmetry properties. Unlike preceding analyses, we assume a barrier of rectangular

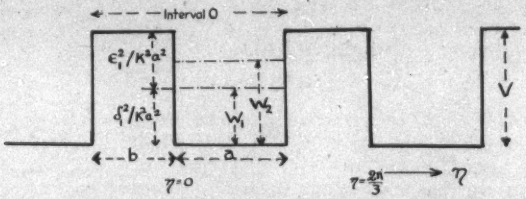

Figure 1. Variation of V with η.

shape. In view of the three-fold symmetry of the rotators, V must repeat at intervals of $2\pi/3$ in η. Thus, if the barriers are each of width b, the valley between them must be of width a, where

$$a + b = 2\pi/3. \qquad \ldots\ldots(7)$$

For convenience, each complete range of $2\pi/3$ is termed an *interval*, the interval of zero order extending from $-b$ to $+a$.

The function $\chi(\eta)$ must be so chosen that it satisfies equation (6) and fulfils the boundary condition $\chi(\eta+2\pi) = \pm\chi(\eta)$; moreover, both χ and $d\chi/d\eta$ must be continuous, even for discontinuous values of V. The form of equation (6) shows that if χ is a solution in a certain angular region η_0, and η_0 is increased by an integral number of times $2\pi/3$, χ and, more generally, $M\chi$ will be a solution in the new region, M being any quantity which is constant within the interval.

One can conveniently express χ for an angle η in the pth interval in terms of the value of χ for the corresponding angle η_0 in the zero interval, multiplied by an exponential chosen as so to fulfil the boundary condition (5)

$$\chi(\eta) = \chi(\eta_0)e^{ipx}, \qquad \ldots\ldots(8)$$

$$\eta = \eta_0 + p\,.\,2\pi/3 \qquad \ldots\ldots(8\,a)$$

and p is an integer. α is defined from the boundary condition

$$\chi(\eta_0+2\pi) = \pm\chi(\eta_0),$$

depending on whether Λ is even or odd. The simplest solutions are where

$$\begin{aligned} &\alpha = 0, \quad 2\pi/3, \quad 4\pi/3; \quad (\Lambda \text{ even}) \\ &\alpha = \pi/3, \quad \pi, \quad 5\pi/3; \quad (\Lambda \text{ odd}) \end{aligned} \qquad \ldots\ldots(9)$$

these solutions include all others.

The wave functions for the first three intervals can thus be expressed in terms of one of them, as shown in table 1. Only solutions for the interval 0 need therefore be found.

Table 1

	Λ even			Λ odd		
Interval (p)	$\alpha=0$	$\alpha=2\pi/3$	$\alpha=4\pi/3$	$\alpha=\pi/3$	$\alpha=\pi$	$\alpha=5\pi/3$
0 ($\eta=\eta_0$)	$\chi(\eta_0)$	$\chi(\eta_0)$	$\chi(\eta_0)$	$\chi(\eta_0)$	$\chi(\eta_0)$	$\chi(\eta_0)$
1 ($\eta=\eta_0+2\pi/3$)	$\chi(\eta_0)$	$\chi(\eta_0)e^{2\pi i/3}$	$\chi(\eta_0)e^{4\pi i/3}$	$\chi(\eta_0)e^{\pi i/3}$	$\chi(\eta_0)e^{i\pi}$	$\chi(\eta_0)e^{5\pi i/3}$
2 ($\eta=\eta_0+4\pi/3$)	$\chi(\eta_0)$	$\chi(\eta_0)e^{4\pi i/3}$	$\chi(\eta_0)e^{8\pi i/3}$	$\chi(\eta_0)e^{2\pi i/3}$	$\chi(\eta_0)e^{2\pi i}$	$\chi(\eta_0)e^{10\pi i/3}$

In the range $0<\eta_0<a$, $V=0$, so that (6) becomes

$$d^2\chi/d\eta_0^2+\beta^2\chi = 0,$$

with the solution $\chi(\eta_0) = Ce^{i\beta\eta_0}+De^{-i\beta\eta_0}$.

The general solution, for an angle η in any interval p, is then found:

$$\chi(\eta)=\chi(\eta_0)e^{i\alpha p}=\{Ce^{i\beta\eta_0}+De^{-i\beta\eta_0}\}e^{i\alpha p}. \qquad \ldots\ldots(10)$$

Similarly, in the interval $-b<\eta_0<0$,

$$d^2\chi/d\eta^2-\gamma^2\chi = 0$$

has a solution, $\chi(\eta_0) = Ae^{\gamma\eta_0}+Be^{-\gamma\eta_0}$,

so that $$\chi(\eta) = \chi(\eta_0)e^{i\alpha p}=(Ae^{\gamma\eta_0}+Be^{-\gamma\eta_0})e^{i\alpha p}. \qquad \ldots\ldots(11)$$

Continuity conditions

For continuity of χ on both sides of the boundary, $\eta = \alpha p$ (i.e. $\eta_0 = 0$),

$$C+D=A+B, \qquad \ldots\ldots(12\,a)$$

and continuity in $d\chi/d\eta$ at the same boundary implies the condition

$$A\gamma-B\gamma = Ci\beta-Di\beta. \qquad \ldots\ldots(12\,b)$$

Continuity of χ at the boundary when $\eta_0=a$ ($\eta=-b+p$. $2\pi/3 = a+(p-1)2\pi/3$) implies

$$\chi(a)e^{i\alpha(p-1)}=\chi(-b)e^{i\alpha p},$$

or $$Ce^{i\beta a}+De^{i\beta a}=(Ae^{-\gamma b}+Be^{\gamma b})e^{i\alpha}, \qquad \ldots\ldots(12\,c)$$

while continuity in $d\chi/d\eta$ at the same boundary leads to the relation

$$Ci\beta e^{i\beta a}-Di\beta e^{-i\beta a}=e^{i\alpha}(A\gamma e^{-\gamma b}-B\gamma e^{\gamma b}). \qquad \ldots\ldots(12\,d)$$

These four equations will only have roots ($\neq 0$) if the following relation holds between the four parameters A, B, C and D:

$$\frac{\gamma^2-\beta^2}{2\beta\gamma}\sinh\gamma b\sin\beta a+\cosh\gamma b\cos\beta a=\cos\alpha. \qquad \ldots\ldots(13)$$

The permitted energy levels may be determined from the solutions of this equation.

To solve the equation for β and γ, the following substitutions may be made:

$$\left.\begin{aligned} \beta a=\delta, \quad \gamma a=\epsilon, \quad y=\gamma/\beta=\epsilon/\delta,\\ K^2Va^2=H^2, \quad s=\cos\alpha\,(=1,\tfrac{1}{2},-\tfrac{1}{2},-1). \end{aligned}\right\} \qquad \ldots\ldots(14)$$

The quantity H^2 determines to a considerable extent the effect of the barrier, while δ^2 and ϵ^2 are respectively proportional to the distance from an energy level to the base and to the top of the potential barrier. The following relations hold:

$$\left.\begin{aligned} \delta^2+\epsilon^2=H^2,\\ \delta^2=H^2/(1+y^2),\\ \delta=\epsilon y, \end{aligned}\right\} \qquad \ldots\ldots(15)$$

while the fundamental equation (13) becomes

$$\frac{y^2-1}{2y}\sinh\gamma b\sin\delta+\cosh\gamma b\cos\delta=s. \qquad \ldots\ldots(16)$$

The value of K^2, assuming a tetrahedral angle at C, a C—H spacing of 1·09 A. and a H mass of $1{\cdot}673\times10^{-24}$ gm., is found to be $4{\cdot}78_8\times10^{14}$.

Pitzer, assuming a cosine form of barrier, obtains a height of about 3000 cal./mole ($2{\cdot}14\times10^{-13}$ ergs or 0·135 ev.). We shall at first deal with barriers above 2000 cal./mole, and deduce the corresponding frequencies. To simplify the calculations we consider primarily those energy levels which lie well below the potential barrier. For these levels we make take $\epsilon\lesssim\delta$. Pitzer's value corresponds to a value of $103\,a^2$ for $K^2Va^2(=\epsilon^2+\delta^2)$, so that $\epsilon\geqslant 7a$. Physical conditions would lead us to assume that a and b are of the same order. Then $\gamma b\sim\gamma a=\epsilon$, so that a probable minimum value for $\sinh\gamma b$ and $\cosh\gamma b$ is about 500. Compared with this figure, the splitting factor s is small, and may be neglected in a first approximation. Detailed calculation will show how far this approximation is valid.

At the same time, we may take $\sinh\gamma b$ and $\cosh\gamma b$ as equal. The equation can now be written

$$\left(\frac{y^2-1}{2y}+\cot\delta\right)\sin\delta\sinh\gamma b=0. \qquad \ldots\ldots(17)$$

Approximate solutions occur for values of δ which satisfy the equation

$$\frac{1-y^2}{2y}=\cot\delta \qquad \ldots\ldots(18)$$

when $\delta^2=H^2/(1+y^2)$.

A simple graphical method of solution which shows the relation between the roots can be obtained by calculating δ for various values of y (a suitable value of H^2 being assumed). Graphs of $(1-y^2)/2y$ and $\cot\delta$ are then plotted against δ, points of intersection giving the required roots. Different sets of solutions can

be obtained by choosing different values for H^2. For any given value of y, the only effect of this change is to multiply δ by a constant factor, and hence to contract or expand the $(1-y^2)/2y$ curve along the δ axis. The $\cot\delta/\delta$ curve of course remains unchanged. It is therefore possible to draw up a series of solutions $\delta_1, \delta_2, \delta_3 \ldots$ determined in terms of the parameter H.

Several interesting conclusions emerge from these results. In the first place δ depends only on H, except, perhaps, for low values of the latter, when the approximations considered may break down. Thus, if H remains constant, the ratio $W/V = \delta^2/H$ will not change. Increases in the barrier height, accompanied by suitable changes in the barrier width, will then cause proportionate increases in the energy levels, the relative heights of the permitted energy levels remaining unaltered.

In the second place, variations in the barrier constant H over a wide range produce only small changes in δ. It is therefore not very satisfactory to deduce H from data on δ.

Finally, the relative values of $\delta_1, \delta_2, \delta_3$ are very nearly in the ratios $1:2:3, \ldots$

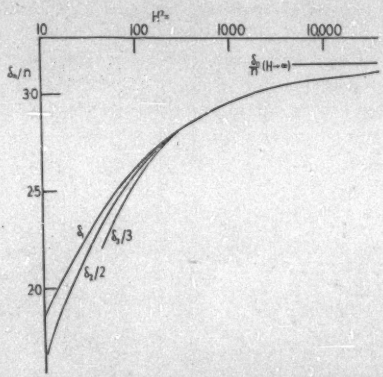

Figure 2. δ_n/n as a function of H^2.

(figure 2), and depart from these ratios by only a few per cent when H^2 reaches values as low as 50. Since it is these relative values which determine the relative heights of the energy levels $W_1, W_2, W_3, \ldots$, one cannot alter the latter greatly by changing the value of the barrier constant H, determined in terms of V and a.

A possible exception may occur when the energy levels considered approach the barrier height. In this case the equation from which they are deduced would in any case require re-examination.

Alternative method

In the case considered above, when $\gamma b \gg 1$, it is possible to solve equation (17) very accurately by the following substitution.

Let $$y = \gamma/\beta = \epsilon/\delta = \tan\theta. \qquad \ldots\ldots(19)$$

Then $$\frac{y^2-1}{2y} = -\cot 2\theta, \qquad \ldots\ldots(19\,a)$$

and $$\cot\delta - \cot 2\theta = s/(\sin\delta \sinh\gamma b); \qquad \ldots\ldots(20)$$

moreover $$\delta^2 = H^2/(1+y^2) = H^2\cos^2\theta, \quad \text{i.e. } \delta = \pm H\cos\theta. \qquad \ldots\ldots(20\,a)$$

(This assumes $\sin\delta \neq 0$ and $\sinh\gamma b \simeq \cosh\gamma b$.)

When the splitting factor s is neglected, solutions of (20) are given by $\delta = 2\theta - n\pi$. It is most convenient to choose any suitable value of δ, and calculate the value of H such that $H = \pm\delta\sec\theta$, where $\theta = (\delta + n\pi)/2$. Barrier constants H corresponding to a series of known energy levels (as determined by δ) can then be readily deduced.

An ambiguity arises, since two values of H can be obtained:

$$H = \pm\delta\sec\delta/2 \quad \text{and} \quad H = \pm\delta\sec(\delta/2 + \pi/2). \qquad \ldots\ldots(21)$$

Only one of these is correct; the other arises from the two possible values $\pm\gamma$, $\pm\beta$, only the squares of which are determined by the given data. Thus if γ and β are of the same sign, only values of θ in the first and third quadrants can be used, while if one chooses γ, β of opposite sign, θ is in the second or fourth quadrant. These two possibilities are included in the $\cot 2\theta$ values, and it becomes necessary to separate the correct solution when inserting in the value for H. If γ, β are both chosen positive, only those values of θ need be considered which lie in an odd quadrant ($\ldots -1, 1, 3, \ldots$).

It is also convenient to choose the sign in (20 *a*) so that H, δ are always positive. This entails the + sign when θ is in quadrants $-3, 1, 5, \ldots$ and the − sign when θ is in quadrants $-1, 3, 7, \ldots$.

§ 3. CALCULATION OF ENERGY LEVELS

For any given value of the barrier constant H, it is possible to determine the relative values of the energy levels, measured from the ground level W_1. To each solution δ_n (or β_n) of (13) or (17) corresponds a permitted level,

$$W_n = \beta_n^2/K^2 = \delta_n^2/K^2a^2 = \delta_n^2 V/H^2, \qquad \ldots\ldots(22)$$

so that $$\frac{W_n - W_1}{W_2 - W_1} = \frac{\delta_n^2 - \delta_1^2}{\delta_2^2 - \delta_1^2}; \qquad \ldots\ldots(23)$$

all energy levels being measured relative to the ground level W_1.

The energy levels of the ethane molecule may be determined from measurements of the specific heat. Kistiakowsky, Lacher and Stitt (1938 and 1939) have shown that energy levels of 0, 275, 520, 725 ... cm.$^{-1}$ account satisfactorily for the observed heat capacity. It is proposed to determine a in terms of H, so that the first energy level ($W_2 - W_1$) shall be that (275 cm.$^{-1}$) observed. Higher levels will then be calculated and compared with those obtained experimentally. At the same time the corresponding value of V may be deduced.

We require a, such that $W_2 - W_1$ or $(\delta_2^2 - \delta_1^2)/K^2a^2 = 275$ cm.$^{-1} = 5{\cdot}45 \times 10^{-14}$ ergs; hence $a = (\delta_2^2 - \delta_1^2)/26{\cdot}1$. Since δ_1, δ_2 are known for any value of H within the range under consideration, a may thus be evaluated in terms of H, so that, whatever the value of H, the first energy-level is always 275 cm.$^{-1}$. The values of a and b/a ($= r$) corresponding to different barrier constants H are shown in figure 3 *a*. Except for comparatively shallow barriers, the variation in a is

slight. It is of interest to note that the value of a obtained is of the expected order ($\pi/3$). An increase of the first energy-level difference from 275 cm.$^{-1}$ to less than 400 cm.$^{-1}$ leads to unacceptable solutions ($a>2\pi/3$). The corresponding potential height V is obtained from the relation

$$V=H^2/K^2a^2.$$

The higher energy levels are determined by means of (23), which may now be written

$$W_n-W_1=\{(\delta_n{}^2-\delta_1{}^2)/(\delta_2{}^2-\delta_1{}^2)\}.275\text{ cm.}^{-1}. \qquad \ldots\ldots(24)$$

For large values of H ($H^2>100$), $\delta_n \propto n$, so that $W_3-W_1=733$ cm.$^{-2}$ This is in direct contradiction with the observed value of about 520 cm.$^{-1}$ (± 10 cm.$^{-1}$). Similarly, the calculated third energy-level difference (W_4-W_1) 1375 cm.$^{-1}$ is far greater than that (725 cm.$^{-1}$) obtained from experiments.

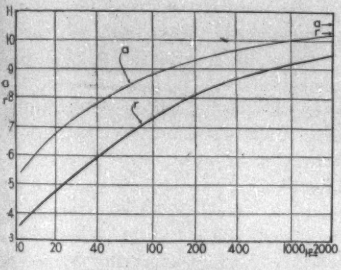

Figure 3 *a*. Relation between a, a/b ($=r$), and H^2 (first energy level$=$275 cm.$^{-1}$).

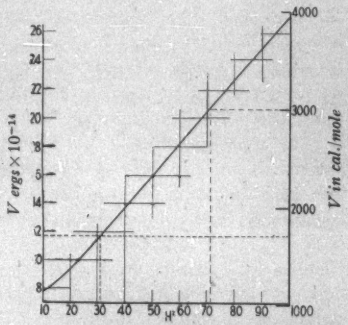

Figure 3 *b*. Relation between V and H (first energy level$=$275 cm.$^{-1}$).

For smaller values of H, W_3-W_1 decreases slowly until a value $H^2\sim 40$ (2000 cal./mole) is reached, when the energy levels considered are of the same height as the potential barrier, and the equation used requires consideration. Even in this case, however, the second energy-level difference does not lie below 600 cm.$^{-1}$

We must therefore conclude that, with the shape of the barrier chosen, and for potential barriers which are not very much smaller than those obtained by Pitzer, no satisfactory agreement can be obtained between the calculated energy levels (with the exception of the first) and those found experimentally. Moreover, for barriers greater than 4000 cal./mole, and of width of about $\pi/3$, the lowest energy levels found are well-nigh independent of barrier height. The energy levels for smaller values of the barrier constant H will be considered below.

§ 4. EFFECT OF SPLITTING FACTOR (s)

In the above work, the influence of s has been neglected, and the solutions obtained are only approximate in nature. The effect of s on the energy levels will be evaluated in two cases, depending on how close to the top of the potential

barrier the energy level considered lies. These two cases correspond to $\gamma \gg 1$ and $\gamma \ll 1$. The intermediate case is less amenable to direct treatment.

(*a*) $\gamma \gg 1$, so that $\cosh \gamma b = \sinh \gamma b$.

This implies that the difference $V - W$ between potential-barrier height and energy level considered is not very small.

The equation $$-\cot 2\theta \sin \delta \sinh \gamma b + \cos \delta \cosh \gamma b = s$$

can, subject to the assumed condition, be written

$$\sin (2\theta - \delta) = s \sin 2\theta / \sinh \gamma b. \qquad \ldots\ldots(25)$$

The approximate solutions previously considered correspond to the values $\delta = 2\theta - n\pi$. To obtain more exact solutions, neighbouring values of θ are chosen, and the corresponding values of δ determined from the condition $\delta = \pm H \cos \theta$, which must be accurately obeyed. γb is then calculated from δ, H and the corresponding ratio b/a. Both sides of the equation are then obtained and compared. The solutions for several values of H are shown in table 2. The splitting of the third level (when $H^2 = 63{\cdot}1$) and of the fourth level (when $H^2 = 100$) shows that the effect is slight even when the energy levels are not far removed from the top of the potential barrier. For levels well below this, the splitting becomes negligible, due to the considerable value of $\sinh \gamma b$.

The apparent ambiguity in θ previously mentioned leads to an interesting alternation in the split levels. Only those values of θ which lie in an odd quadrant are acceptable (γ, β positive), so that $\sin 2\theta$ is always positive. Consider two consecutive levels, corresponding to values of δ differing by about π. Then if $2\theta = \delta_1$ is one acceptable solution (θ in odd quadrant), and δ_1 is increased by about π to δ_2, 2θ must be increased not by π, but 2π, if θ is still to remain in an odd quadrant. Thus in successive levels $\sin (2\theta - \delta)$ will reverse sign, whereas the sign of $s \sin 2\theta / \sinh \gamma b$ is unaffected. The effect of a given splitting factor s will, in alternate levels, be to increase or decrease the energy levels. This appears more clearly in the limiting case considered below.

Table 2. Split levels ($\delta < H$)

H^2	b (approx.)	$s=$ 1	$\frac{1}{2}$	0	$-\frac{1}{2}$	-1
100	2·5–2·8	$9{\cdot}84_2$	$9{\cdot}82_7$	$9{\cdot}81_2$	9·80	$9{\cdot}78_8$
63·1	5·2	$7{\cdot}16_7$	$7{\cdot}17_0$	$7{\cdot}17_2$	$7{\cdot}17_5$	$7{\cdot}17_8$

(*b*) $\gamma \ll 1$.

The energy levels considered are nearly equal to, but are still less than, the potential barrier height V. The case when they are greater than V will be dealt with later. The approximation $\sinh \gamma b = \cosh \gamma b$ is no longer valid. Equation (13) can be simplified ($\gamma^2 \ll \beta^2$ $(\gamma b)^2 \ll 1$) and becomes

$$-\frac{\delta}{2} \cdot \frac{b}{a} \sin \delta + \cos \delta = s. \qquad \ldots\ldots(26)$$

The values for δ_n, when this is nearly equal to H, are $(n-1)\pi$ approximately. Let $\delta_n = (n-1)\pi + f$, and assume $\sin f \sim f$, $\cos f = 1$. Then replacing in (26) and simplifying $f = 2\{1 - s(-1)^{n-1}\} \cdot a/b(n-1)\pi$,

$$\delta_n = (n-1)\pi + \frac{2a}{b(n-1)\pi}\{1 - s(-1)^{n-1}\}. \qquad \ldots\ldots(27)$$

The factor $\{1-s(-1)^{n-1}\}$ produces an alternation in the split levels, depending both on the level considered n and on the solid-body rotation quantum number Λ. Table 3 shows the maximum splitting for levels below the potential barrier height. The values obtained are, however, only approximate, since f is

Table 3. Maximum splitting ($\delta^2 \ll H^2$)

H (approx.)	n	a/b	$\frac{2a}{b(n-1)\pi}$	δ					
				Λ even			Λ odd		
3·2	2	0·31	0·2	3·54	3·24	3·24	3·14	3·44	3·44
6·4	3	0·59	0·188	6·28	6·56	6·56	6·65	6·37	6·37
9·6	4	0·71	0·15	9·72	9·50	9·50	9·42	9·64	9·64

not sufficiently small to justify the approximation made. Moreover, the split values of δ_n thus obtained do not relate to a single value of H, but to that value of H for which the energy level equals the barrier height. The actual splitting is appreciably smaller.

§ 5. SPECIAL CASES

A number of assumptions have been made as to the relative importance of different factors. These may now be examined to discover whether the approximations made are valid under the conditions concerned.

In the general solution we have taken $\cosh\gamma b = \sinh\gamma b$. A more accurate analysis, but still neglecting the splitting factor, leads to the equation

$$\frac{1-y^2}{2y} = \cot\delta \coth\gamma b.$$

In the graphical solution the $\{1-y^2/2y, \delta\}$ curve will remain unaltered, while the $\{\cot\delta, \delta\}$ curve will have ordinates reduced by a variable factor $\coth\gamma b$. Since b is of the order of unity, the correction will only be appreciable for values of γ of about 2 or less. Only energy levels very nearly equal to the potential-barrier height will be affected. Moreover, if $\gamma b = 1$, the ordinates of the $\{\cot\delta, \coth\gamma b\}$ curve are decreased by 24 %. The solutions of the modified equation will then change, but by an amount considerably less than this, due to the steepness of the $\{\cot\delta, \delta\}$ curve. Thus, even in this extreme case, where $W/V > 0{\cdot}9$, the values of δ will differ from those obtained from the simpler equation $(1-y^2)2y = \cot\delta$ by less than 2 %. We are therefore justified in neglecting the $\coth\gamma b$ correction term in all but the most exceptional cases.

In certain cases the splitting factor s ($\neq 0$) may produce appreciable changes in the energy levels. These generally occur when the value of $\cosh\gamma b$ and $\sinh\gamma b$ are sufficiently small, i.e. for energy levels nearly equal to the potential height V. These have already been considered. A more special case occurs when in the general equation

$$\frac{y^2-1}{2y}\sin\delta\sinh\gamma b + \cos\delta\cosh\gamma b = s$$

(where γb is quite large) the left-hand side is small because both $\cos\delta$ and $(y^2-1)/2y$ are nearly zero. In this way the left-hand side may become comparable to unity in spite of the large values of $\sinh\gamma b$, $\cosh\gamma b$; s is no longer negligible, and may then exert an appreciable effect on energy levels which are by no means comparable with V.

It may be readily shown that the effect of the splitting factor s is to change δ by a very small amount, of the order of $s/\sinh\gamma b$. The splitting effect of s does not therefore differ appreciably from the general case.

§6. ENERGY LEVELS FOR LOW BARRIERS

Until the present moment we have limited the investigation to barriers of height greater than that defined by $H^2=40$, and to energy levels below the barrier height. This is sufficient to show that a 3000 cal./mole barrier can only succeed in correlating the experimental data if, in addition, a cosine shape of potential function is assumed. We shall now extend the work to lower barriers, and consider whether a suitable choice of potential height and breadth cannot be found to afford equally good agreement. Moreover, the splitting produced by the threefold degeneracy of the system will likewise be investigated.

For levels whose height W exceeds the potential-barrier height V, the difference $V-W$ or γ^2 is negative. Putting $\gamma^2=-\lambda^2$ in equation (13) and simplifying gives the alternative formula:

$$-\frac{\lambda^2a^2+\delta^2}{2\lambda a\,.\,\delta}\sin\lambda b\,.\sin\delta+\cos\lambda b\cos\delta=s. \qquad \ldots\ldots(28)$$

Solutions of this equation are not readily found as previously. A number of special solutions were obtained by putting $\delta=n\pi$, $(n+\frac{1}{2})\pi$, etc. The values of λa could then be obtained for any desired value H^2,

$$\delta^2-(\lambda a)^2=H^2, \qquad \ldots\ldots(29)$$

and the corresponding values λb deduced from the known ratio b/a. Since this ratio is obtained from the first two energy levels, whose values have only been obtained for $H^2>9{\cdot}8$, the calculations have been limited to greater values of H than $3{\cdot}1$.

Consider the variation of the function for $\delta=2\pi$. The equation then becomes $\cos\lambda b=s$. Solutions neglecting splitting ($s=0$) are $\lambda b=\pi/2$, $3\pi/2$, etc., and from these values the corresponding values H can be obtained. The split levels for $s=\pm\frac{1}{2}$ fall on either side of the non-split level, ($\lambda b=\pi/3$, $2\pi/3$, etc.), while for $s=\pm1$ the split levels from adjacent levels overlap completely ($\lambda b=0$, π, 2π, etc.).

To discover whether this overlap occurs for all values of δ, the graph of the function

$$s=-\frac{(\lambda a)^2+\delta^2}{2\lambda a\,.\,\delta}\sin\lambda b\sin\delta+\cos\lambda b\cos\delta \qquad \ldots\ldots(30)$$

can be considered as a function of δ, keeping H constant. Two typical cases ($H^2=14$, $H^2=31$) are shown in figure 4. The values of δ can be directly obtained for values of s equal to 1, $\frac{1}{2}$, $-\frac{1}{2}$, -1. It will be seen that the degeneracy produced at $\delta=2\pi$ is only exceptional. The large values assumed by s in the

transition from $\delta > H$ to $\delta < H$ are also clear. Nevertheless the splitting effect of s is very marked for all the range considered, and it becomes difficult to correlate the assembly of six split levels with a single level as required for the experimental

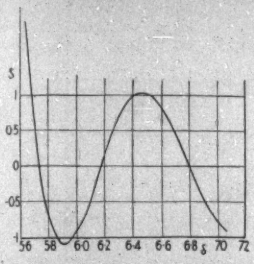

Figure 4 *a*. Plot of s as function of δ ($H^2=31$).

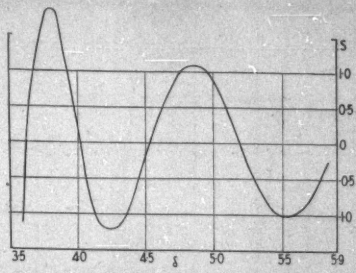

Figure 4 *b*. Plot of s as function of δ ($H^2=14$).

results. We have taken the average value of a level as that corresponding to $s = 0$. For $H^2 = 31$, the mean energy levels relative to the ground level are shown below (table 4), as well as the split levels from which they arise.

Table 4. Energy levels for 1700 cal./mole; 1·35 radians barrier ($H^2 = 31$)

Energy levels ($cm.^{-1}$) relative to ground level									
First	Second	Third				Fourth			
0	275	523				621			
Splitting negligible	negligible	511	516	531	549	679	645	602	576
Splitting factor		1	$\frac{1}{2}$	$-\frac{1}{2}$	-1	1	$\frac{1}{2}$	$-\frac{1}{2}$	-1

When A is even, each level is tripled ($s=1$, $s=\frac{1}{2}$, $s=\frac{1}{2}$); when A is odd, the levels correspond to $s=-1$, $\frac{1}{2}$, $\frac{1}{2}$.

The logarithmic plot of $\log\delta$ against $\log H$ for levels above V is almost linear. Over the region $12 < H^2 < 60$ the deviation from this law is of the order of several per cent. Then we may put approximately

$$\delta = CH^m,$$

where C, m are constants which may be determined for each level and have the approximate values

third level:	$C = 1{\cdot}22_7$	$m = 0{\cdot}89_8$
fourth level:	$C = 1{\cdot}65_2$	$m = 0{\cdot}76_8$
fifth level:	$C = 2{\cdot}11_3$	$m = 0{\cdot}68_0$

Over the range of values under consideration the first two levels W_1, W_2 remain less than V, and δ_1, δ_2 may hence be determined as before. This suffices to determine a as a function of H. Further solutions for δ_3, δ_4 may then be

obtained as above (figure 5), and the corresponding energy levels W_3-W_1, W_4-W_1, etc., calculated. Best agreement with observed values is obtained for $H^2=31$. In table 5 the energy levels are calculated for various rectangular

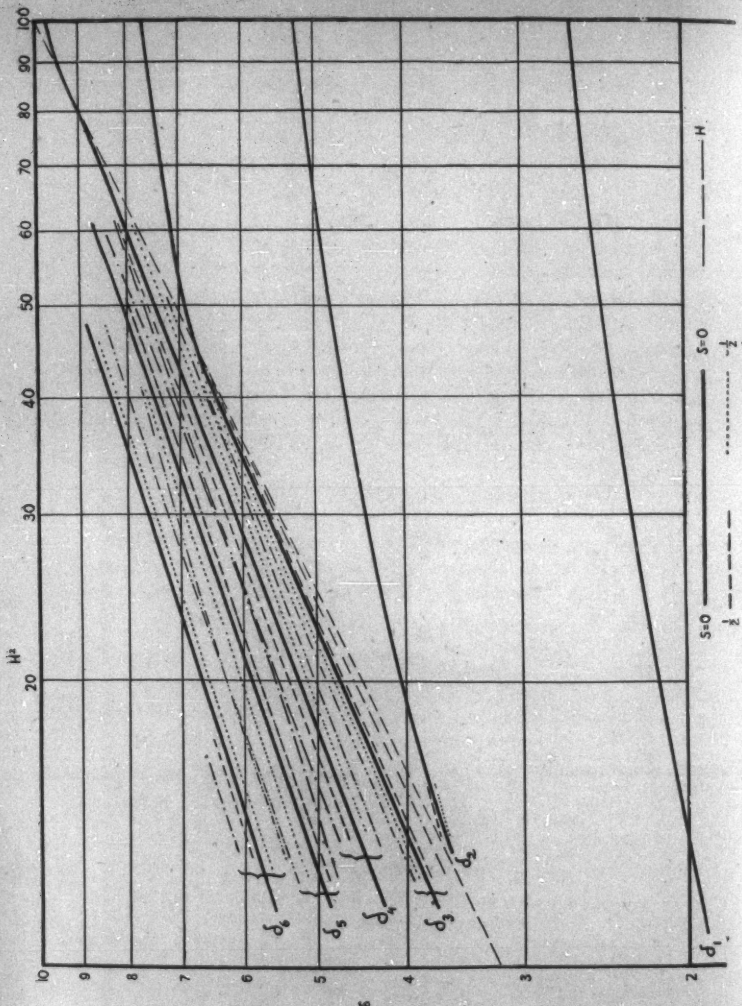

Figure 5. δ as a function of H (a adjusted so that $W_2-W_1=275$ cm.$^{-1}$).
If $W \gtrsim V$, energy-level splitting shown. A even: $s=1, -\frac{1}{2}, -\frac{1}{2}$.
A odd: $s=-1, \frac{1}{2}, \frac{1}{2}$.
If $W \ll V$, energy-level splitting is inappreciable.

barriers. In the case of the 3000 cal./mole barrier, the height is that deduced by Pitzer for a cosine barrier, while the width is chosen to reproduce the 275 cm.$^{-1}$ level. It will be noted that the other levels are then not reproduced satisfactorily.

Table 5. Comparison of energy-level differences (rectangular barrier)

H^2	Barrier height (cal.)	Barrier width (radians)	Energy level ($cm.^{-1}$)	Levels above ground ($cm.^{-1}$)	
∞	∞	1·03	275	733	1357
71·5	3000*	1·25	275	704	
31	1700	1·35	275	523	621
29	1630	1·35	275	503	596
†	—	—	275±10	520±10	725±?

* Value obtained by Pitzer (cosine barrier).
† Value obtained by Kistiakowsky and colleagues (specific-heat measurements).

The effect of A on the torsional energy levels is shown in figure 6 for the case of $H^2=31$ (1700 cal./mole barrier). The energy levels W are, of course,

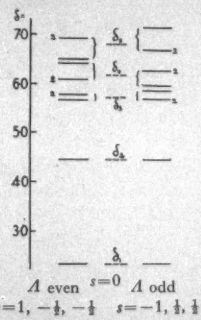

Figure 6. Splitting of levels for $H^2=31$. (δ_1, δ_2, splitting is inappreciable) δ_3, δ_4, δ_5 consist of one singlet and one doublet.

proportional to δ^2. The mean values for any level (i.e. assuming $s=0$) are shown in broken lines.

§ 7. APPLICATION TO C_2D_6

From the values of V and a deduced for ethane, it is possible to calculate the levels which should be observed in hexadeuteroethane, assuming the barrier constants to remain unaltered. The only change is then due to increased inertia, $I_D=2I_H$, so that $K_D^2=2K_H^2=9{\cdot}57_6\times10^{14}$ and $H_D^2=2H_H^2$.

It was found above that satisfactory agreement could be obtained for $H_H^2=31$ in the case of ethane. The energy levels for $H_D^2=62$ should therefore be investigated. The values of δ obtained as solutions of (13) are given in table 6. The corresponding energy levels W are then deduced from the expression

$$\delta^2=K_D^2Wa^2,$$

If $W<V$, energy-level splitting is inappreciable.

where K_D^2 has the value given above, and a is to be taken equal to the value deduced for the case of ethane ($H_H^2 = 31$, $a = 0{\cdot}74_5$). As a check, the levels

Table 6

$V = 1700$ cal./mole $\quad b = 1{\cdot}35 \quad a = 0{\cdot}745$

C_2D_6 ; $H^2 = 62$			C_2H_6 ; $H^2 = 31$		
δ	Energy ($cm.^{-1}$)	Energy levels * ($cm.^{-1}$)	δ	Energy (cm^{-1})	Energy levels * ($cm.^{-1}$)
$2{\cdot}50$	59	0	$2{\cdot}29$	99	0
$4{\cdot}9_3$	231	172	$4{\cdot}43$	373	274
$7{\cdot}1_2$	481	432	$5{\cdot}73_5$	625	524

* Values relative to ground level.

for ethane were deduced at the same time. The values thus obtained differ by about 1 $cm.^{-1}$ from those obtained more directly.

§8. CONCLUSION

The height of the potential barrier restricting free rotation in ethane has previously been evaluated assuming that the variation with angle of torsion follows a sinusoidal law, in which the only parameter is therefore the height V. If the maximum variation in V is taken as about 3000 cal./mole, satisfactory agreement is established between theoretical and experimental results. The lowest energy levels of the torsional vibration which are obtained from specific-heat measurements agree well with those deduced theoretically from the 3000 cal./mole barrier.

If, however, a barrier of the same height but of different (rectangular) shape be assumed, the energy levels no longer agree satisfactorily with experimental evidence. Nor can this agreement be obtained by variation in the barrier dimensions (height and width) except for the considerably lower value 1700 cal./mole (barrier width = 1·35 radians). Agreement is then again obtained with the experimental values, although the fourth energy level is somewhat less satisfactory than for the cosine functions. This could probably be improved, however, by small variations in the initial energy levels used in the calculations, since these are only known approximately. Moreover, the effect of splitting of the higher levels is found to be very considerable.

The energy levels for the heavy-ethane molecule have been calculated on the basis of the barrier dimensions found suitable in the case of ethane. Comparison with the values deduced by Kistiakowsky and colleagues is, however, impossible, since these have not been published. Barriers below 1100 cal./mole in the case of ethane have not been investigated.

It appears from these results that there is no decisive evidence for a cosine form of barrier. Moreover, the estimated height (3000 cal./mole) is only significant in so far as a barrier of this shape is assumed. The figure obtained

should therefore only be treated as a convenient means of comparing data for different molecules.

More detailed experimental evidence is therefore required before an accurate estimate of the shape and dimensions of the potential barrier can be obtained.

§ 9. ACKNOWLEDGEMENT

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THE THERMAL EXPANSION OF ALUMINIUM: FURTHER EXPERIMENTS

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ABSTRACT. It is found that the double curvature previously reported in the thermal expansion coefficient of aluminium is characteristic of unannealed filings. After the filings have been heated above 600° C., it disappears. The following values are obtained for the expansion coefficient of annealed filings :

t	0°	100°	200°	300°	400°	500°	600° C.
$10^6\alpha$	22·8	24·0	26·0	28·3	30·3	32·7	35·0

Certain observations on the effect of cold-work are recorded.

§ 1. INTRODUCTION

THE writer's previous measurements of the expansion of aluminium (Wilson, 1941) were somewhat severely criticized in a review in the *Metallurgist* (27 June 1941). The chief grounds for the criticism were (*a*) that the results were not in accord with those of previous work, and (*b*) that no details of the annealing treatment were given. The first objection is not

very important, particularly as the two determinations specifically cited as in good agreement (Taylor, Willey, Smith and Edwards, 1938; Esser and Eusterbrock, 1940–41) differ by about 7 % at 500° c.; it was, however, thought desirable to investigate the effect of annealing.

It has usually been considered that cold-working has little effect on the properties of pure aluminium. Tammann (1934), after describing changes produced by cold-working in the electrical resistance, thermal e.m.f. and other properties of silver, gold and copper, states that aluminium behaves differently in that the electrical resistance and thermal e.m.f. are unchanged, and that the x-ray reflections are not broadened. Wood (1939) found that moderate cold-working increased the lattice parameters of silver, copper, iron, nickel and molybdenum; extreme cold-working reduced them below those of the undistorted metals. Aluminium, on the other hand, was "spontaneously self-recovering during cold-working, and for this reason was found not to exhibit the changes described for copper and silver". Thomassen and Wilson (1933) found, however, that reflections from aluminium worked at the temperature of solid CO_2 were broader than reflections from aluminium worked at room temperature.

In order to see if the writer's S-shaped (α, T) curve could be explained by lack of annealing, the spacing measurements on which it was based were divided into two sets: those taken before and those taken after the aluminium had been heated above 600° c. It was found that the former set showed the double curvature; the latter did not, but there was no observation at the temperature (100°) most important for establishing it. It also appeared that there was a small difference at 0° c. between the lattice parameters of heated and unheated aluminium filings.

§ 2. MEASUREMENTS OF LATTICE PARAMETER

Further measurements were therefore made: they and the previous measurements are given in table 1. Both the old and the new measurements are divided

Table 1. The lattice parameter of aluminium

Temp. (° c.)	Lattice parameter					
	Unannealed filings		Annealed filings			
	OM	NM	OM	NM, OS	NM, NS	Mean, NM
0	4·038 9_1[b]	4·038 5_3[C]	4·039 1_5[d]	4·039 1_5[a]	4·039 1_2[C]	4·039 1_5
50	—	—	—	4·043 7_0	—	4·043 7_0
100	4·048 5_5	4·048 3_3	—	4·048 5_0	4·048 5_7	4·048 5_4
150	4·053 8_2	4·053 5_2	—	4·053 4_3	4·053 3_0	4·053 3_6
200	4·058 9_0	4·058 7_5	4·058 9_0	4·058 6_5	4·058 5_5	4·058 6_0
300	4·070 1_0	4·069 6_5	4·069 9_4	4·069 7_4	4·069 7_3	4·069 7_4
400	4·081 9_7	4·081 9_1	4·081 9_0	4·081 6_0	4·081 6_7	4·081 6_4
500	4·094 6_8	—				
600	4·108 6_3	—	4·108 6_8[e]			
650	—	—	4·116 1_8[c]			

OM = Old measurements
NM = New measurements
OS = Old specimen
NS = New specimen
C = Cold-rolled sheet

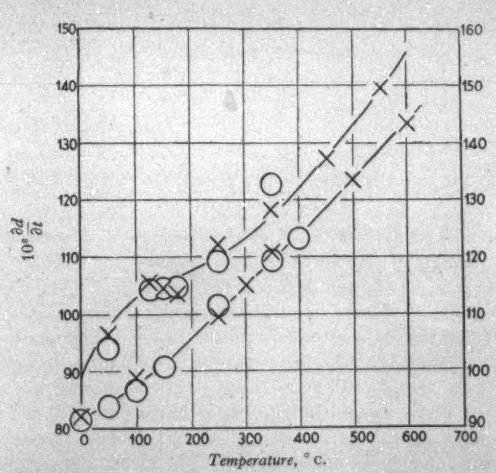
a = Mean of two
b = Mean of three
c = Mean of four
d = Mean of five
e = Mean of six

into two groups: those made before and those made after heating above 600° c. The experimental technique for the new measurements was as described by Stokes and Wilson (1941). The thermocouple was calibrated at a further fixed point: the melting point of zinc (419·45° c.).

It is rather disappointing to find that the new results from 100° upwards are, on the average, 0·000 3 A. lower than the old. In the previous paper it was estimated that the total error, both from the measurement of temperature and from the x-ray technique, was not greater than 0·000 2 A., but it appears that this was over-optimistic. The new measurements are probably the more reliable as the thermocouple was calibrated at a greater number of points.

§3. THE THERMAL EXPANSION COEFFICIENT

The mean values of $10^6\,\partial d/\partial t$ over intervals of 50° or 100° from both the old (×) and the new (○) measurements on unannealed aluminium filings are plotted in the figure (upper curve: scale on left). Both show a double curvature with a point of inflection at approximately 150° c. This is in general agreement with the curve published previously (full line). The values obtained from both the old (×) and the new (○) measurements on filings after heating to 600° are shown in the lower curve (scale on right) of the same figure. In addition to the



Slope of spacing—temperature curve. Upper curve, left scale, unannealed filings. Lower curve, right scale, annealed filings. ×, old measurements; ○, new measurements.

means, values of $10^6\,\partial d/\partial t$ at the ends of the ranges are also plotted. These were calculated by the method of Bickley and Miller (1942), the first three differences being used. There is no sign of the double curvature shown by the upper curve. In view of the above-mentioned difference between the old and the new measurements, a difference in expansion between annealed and unannealed filings cannot be regarded as certain: it is, however, suggestive that both sets of measurements indicate a double curvature in the expansion coefficient of unannealed filings.

The coefficient of expansion, α, was obtained by dividing values of $\partial d/\partial t$ from the lower curve in the figure by the corresponding values of d. It is given in the first line of table 2. In the succeeding lines, values of $(1/l)(\partial l/\partial t)$ from the work of Hidnert (1925), Bollenrath * (1934), Taylor *et al.* (1938), Esser and Eusterbrock (1940–41) and Nix and MacNair (1941) are given. The present results are in excellent agreement with those of Hidnert and of Taylor *et al.*,

Table 2. The thermal expansion coefficient of aluminium, $\times 10^6$

Author(s)	Temp. 0°	100°	200°	300°	400°	500°	600°	% Al
Wilson	22·8	24·0	26·0	28·3	30·3	32·7	35·0	99·99
Hidnert	22·6	24·5	26·4	28·3	30·3	32·2	34·1	99·95
Bollenrath	22·8	23·6	25·0	26·6	28·9	31·5	—	99·87
Taylor *et al.*	23·2	24·3	25·9	27·9	30·4	33·4	—	99·996
Esser & Eusterbrock	23·4	24·5	25·8	27·4	29·2	31·3	33·7	99·87
Nix & MacNair (read from graph)	22·9	25·0	26·4	27·7	29·0	(30·3)	(31·6)	99·997

the largest deviation of any of the three determinations from the mean being less than 2 %. Nix and MacNair's results agree well up to 300°, but their theoretical curve, based on a modification of Grüneisen's law, is low at higher temperatures. Its curvature is in the sense opposite to that observed. Bollenrath's results * are low above 100° and Esser and Eusterbrock's above 200°.

§ 4. THE EFFECT OF COLD-WORK ON THE LATTICE PARAMETER

It will be noticed that the mean of the spacings of unannealed filings at 0° C. (4·038 9_1 A.) is 0·000 2 A. lower than the mean of the spacings of annealed filings (4·039 1_5 A.). The mean deviation is in each case 0·000 05 A. By any of the ordinary statistical tests the difference is significant, but in view of the difference between the old and the new results at higher temperatures it might be thought unwise to attach great importance to it. To produce a more definite effect, some of the aluminium was cold-rolled and the spacing of a small sliver measured. The result was 4·038 5_3 A. After heating to 220° it increased to 4·039 1_2 A. It would appear, therefore, that cold-work does produce an appreciable effect on the lattice parameter of aluminium. The Debye-Scherrer lines on the initial photograph of the cold-rolled sheet were slightly broader than those on the photograph obtained after heating. The broadening would be explained by a reduction of the crystallite size to about 1500 A., which is the order of magnitude found by Wood (1939) for other metals, but there are, of course, other explanations. A variation of the lattice parameter over a range of $\pm 0{\cdot}001$ A. would give about the same effect. Such a variation would be consistent with the observations of Wood and Smith (1941).

* It has been assumed that Bollenrath's table gives $(1/l)(\partial l/\partial t)$. It is possible, however, that it is $(1/l_0)(\partial l/\partial t)$.

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THE THEORETICAL VALUES OF THE PHYSICAL CONSTANTS

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ABSTRACT. The principal physical constants are calculated by the theory developed in *Relativity Theory of Protons and Electrons* and subsequent papers, and are compared with the values given by Birge (1942) in *Reports on Progress in Physics*, **8**. There is satisfactory agreement with Birge's values as they stand. But the theory also indicates that small corrections are required in the computation of certain constants from experimental data ; when these corrections are included, the small discordance between the spectroscopic and deflection values of $e/m_e c$ disappears, and the agreement of observation and theory is complete. It is concluded that there ought to be no difference between the direct and indirect values of h/e. The calculated constant of gravitation is $6{\cdot}6665.10^{-8}$; and it is pointed out that the expected agreement of the calculated and observed values is not affected by the mean chemical constitution of the universe or the free radiant energy in space—a point previously left doubtful. As the purpose is not to justify or explain the theory but to compare it with observation, theoretical explanation is limited to the points which arise in adapting the theory to practical comparisons.

§1. INTRODUCTION

FROM the theory developed in *Relativity Theory of Protons and Electrons* (hereafter quoted as *P. & E.*) and subsequent papers (Eddington, 1937, 1940, 1940 a) it should be possible to determine precise values of the general physical constants. The recent publication of R. T. Birge's comprehensive survey of the experimental data (Birge, 1941) provides a suitable opportunity for tabulating the results and comparing them with observation.

A complication arises because the experimental values are expressed in three traditional units—the centimetre, gram and second—which have no relation to any theory. It is therefore necessary to select three measured quantities which, taken together, determine the centimetre, gram and second, to be used as " conversion constants ". They must be quantities which occur in the most

fundamental part of physics, where the theoretical development is believed to be exact (a condition which excludes the characteristics of elements other than hydrogen); subject to this, they should be the most accurately ascertained constants available. Happily there is no doubt as to the most suitable choice. The three quantities are the velocity of light ($\mathbf{c}$), the Rydberg constant for hydrogen ($\mathfrak{R}$) and the Faraday constant for hydrogen ($\mathfrak{F}$). The last-named is the ordinary Faraday constant divided by the atomic weight of hydrogen, so as to reduce it to the scale $^1H=1$ instead of $^{16}O=16$. Birge gives—

Table 1

$\mathbf{c}=(2{\cdot}99776\pm{\cdot}00004)\,.\,10^{10}$ cm. sec.$^{-1}$
$\mathfrak{R}=109677{\cdot}58\pm{\cdot}05$ cm.$^{-1}$
$\mathfrak{F}=9573{\cdot}56\pm1{\cdot}0$ e.m.u.g.$^{-1}$

From table 1 it would be possible to reconstruct the C.G.S. system if the original standards were destroyed. We accordingly adopt it as a *verbal definition* of C.G.S. units which is in as close agreement as possible with the primary *gesticulatory definition*.*

The calculated values of the physical constants, listed later, involve no observational data other than the three conversion constants. The uncertainty of any constant is therefore immediately evident from its dimensions in $\mathbf{c}$-$\mathfrak{R}$-$\mathfrak{F}$ units. The dimensions of the principal quantities are found to be

length	$\mathfrak{R}^{-1}$	angular momentum	$\mathfrak{R}^{-2}\mathfrak{F}^{-2}\mathbf{c}$
time	$\mathfrak{R}^{-1}\mathbf{c}^{-1}$	electrostatic charge	$\mathfrak{R}^{-1}\mathfrak{F}^{-1}\mathbf{c}$
mass	$\mathfrak{R}^{-1}\mathfrak{F}^{-2}$	constant of gravitation	$\mathfrak{F}^{2}\mathbf{c}^{2}$

If Birge's results are accepted, the uncertainties of $\mathbf{c}$ and $\mathfrak{R}$ are negligible, and $\mathfrak{F}$ has a probable error of 1 part in 10 000. The highest power of $\mathfrak{F}$ in any of the listed constants is $\mathfrak{F}^2$; their probable errors should therefore not exceed 1 part in 5000.†

A more serious complication arises because the so-called "observational values" of the constants are not derived wholly from observation, but are a combination of observational data with a theory which I regard as superseded. Thus a straightforward comparison with Birge's constants would not be an observational test of my theory, but a comparison partly with observation and partly with the theory which it condemns. It has to be recognized that the current procedure of computing constants from observational data has acquired the status of a vested interest: and, as far as practicable, we accept it as furnishing the official definition of the constants. We therefore go out of our way to adapt the theoretical formulae (§ 2) to the official definition—so that the calculated constants represent the results which Birge presumably would have obtained by his procedure of reduction if the observational data had been perfect. But the current procedure is not entirely self-consistent, and in the most accurate comparisons we cannot avoid applying small corrections to certain of Birge's

* By a "gesticulatory definition" I mean pointing to an object and saying that the metre (kilogram, day) is the extension (inertia, period) of *that* object.

† Except the last three constants in table 3, for which the theory has not yet been extended to this degree of refinement.

values in order to obtain a genuine comparison of theory and observation. This inconsistency of the official definitions is perhaps rather fortunate, since it relieves us of the obligation of carrying respect for tradition to an embarrassing extreme.

§ 2. THE THEORETICAL FORMULAE

The formulae used in calculating the principal constants are—

(1) $e^2 = \frac{1}{137}\frac{hc}{2\pi}$, (2) $\mathrm{M} = \frac{136}{10}m_0$, (3) $\mu' = \frac{1}{136}m_0$,

(4) $\mathrm{M} = m_p + m_e$, (5) $\mu = m_p m_e/(m_p + m_e)$,

(6) $\mu = \beta^{\frac{1}{2}}\mu'$, (7) $\beta = \frac{137}{136}$,

(8) $m_0 = \frac{\frac{3}{4}\beta^{\frac{1}{2}}h\sqrt{(\frac{3}{5}N)}}{2\pi R_0 c}$, (9) $\frac{N_0}{R_0} = \frac{\pi c^2}{\kappa \mathrm{M}}$,

(10) $N_0 = \frac{3}{4}N$, (11) $N = 2\times 136\times 2^{256}$,

(12) $\kappa = \frac{136\,.\,137}{10}\,\frac{\pi\,\mathfrak{F}^2 c^2 \beta^{\frac{1}{2}}}{\sqrt{(\frac{3}{5}N)}}$,

(13) $\mathfrak{F} = e/\mathrm{M}c$, (14) $\mathfrak{R} = \frac{1}{2}\left(\frac{1}{137}\right)^2\frac{\mu c}{h}$.

Most of the notation is defined in table 3. Of the remaining symbols, c, $\mathfrak{R}$, $\mathfrak{F}$ have already been defined: and m_0 (the mass of a comparison particle), N (the cosmical number) and μ' may here be regarded merely as auxiliaries in the calculation.

Formulae (1), (2), (3), (4), (5), (8), (9) are *P. & E.* (15.65), (12.63), (12.62), (14.71), (14.72) with certain changes explained below; formulae (6), (7), (10) relate to these changes. The result (11) is given in *P. & E.*, §§ 16.5, 16.7. Formula (12) is redundant, being derivable from the others, but is given here as the most direct expression for the constant of gravitation κ in terms of atomic constants. Formula (13) follows from the definition of $\mathfrak{F}$, since M is the mass of a hydrogen atom: and (14) is the ordinary expression for the Rydberg constant simplified by using (1).

From formulae (2)–(6) we find that m_p, m_e are the roots of the equation

$$10m^2 - 136mm_0 + \beta^{\frac{1}{2}}m_0^2 = 0. \qquad \ldots\ldots(15)$$

Apart from the β factors, (12) and (15) are *P. & E.* (14.75) and (12.47).

The changes that have been made in the *P. & E.* formulae are: firstly, a factor $\frac{3}{4}$ has been inserted in (8) and (10). This emendation was announced tentatively by Eddington and Thaxton (1940). I have since been able to simplify the theory, and it has become evident that the factor was wrongly omitted in my earlier work. Both in (8) and (10) it is introduced in reducing the spherical space of relativity theory to the flat space postulated in quantum theory. The number N_0 of elementary particles is now $\frac{3}{4}$ of the cosmical number N, which stands for the number of independent quadruple wave functions at any point. These wave functions are non-integrable in spherical space: and it is the non-integrability that causes the break-down of the simple exclusion principle which assigns one particle to each wave function.

Secondly, coefficients depending on the *Bond factor* $\beta = 137/136$ appear in (6) and (8), and consequentially in (12) and (15). In principle β^{-1} has the same origin as the factor $\frac{3}{4}$, both being of the form $n/(n+1)$ and arising out of the extension of a probability distribution from n to $(n+1)$ dimensions. The additional variate is introduced, in the one case when we represent the inertial-gravitational field quantum-mechanically instead of by an extraneous concept (curvature), and in the other case when we represent the electric field quantum-mechanically by an interchange co-ordinate. The primary introduction of β has already been noticed in *P. & E.*, §§ 15.8, 15.9; but the formulae there given correspond to $\mu = \beta\mu'$. Here the same factor is distributed, $\beta^{\frac{1}{2}}$ being included in (6) and $\beta^{\frac{1}{2}}$ in (8). This change is not an amendment of the theory, but an adaptation of it to give the officially defined constants instead of the constants which the theory itself points out as physically significant. It would be unsuitable in this paper to enter deeply into theoretical questions: but, since the β factors especially concern the comparison of theory with observation, we shall in § 6 go far enough to show how the factors $\beta^{\frac{1}{2}}$, $\beta^{\frac{1}{2}}$ have been determined.

The calculation of the constants proceeds as follows:—From (2), (3), (6) we obtain M/μ. Combining this with (14) we obtain h/Mc. Since (13) gives e/Mc, we obtain h/e: and by (1) we can then find h and e separately. The remaining steps are straightforward.

From R_0 we derive an interesting constant V_0, which is the limiting speed of recession of the galaxies. The well-known formula is

$$V_0 = \mathbf{c}/R_0\sqrt{3} \text{ cm. sec.}^{-1} \text{ per cm. distance.} \qquad \ldots\ldots(16)$$

In table 3, V_0 is given in the usual astronomical measure, km. sec.$^{-1}$ per mega-parsec. Thanks to H. S. Jones's recent determination of the solar parallax, the result remains accurate to 1 part in 10 000. The observed speed is (as it should be) rather less than the calculated limiting speed; but the most optimistic estimate cannot put the probable error at less than 5 per cent.

The theory (Eddington, 1937) also gives two constants important in nuclear physics, namely the constants k and A in the expression

$$E = -Ae^{-r^2/k^2} \qquad \ldots\ldots(17)$$

for the non-Coulombian energy of two protons distant r apart. The formulae are

$$k = \frac{R_0}{\sqrt{N_0}}. \qquad A = \left(\frac{4}{3}\right)^{\frac{1}{2}} \frac{4}{\pi^{\frac{1}{2}}} \frac{m_p}{m_0} \frac{e^2}{k}. \qquad \ldots\ldots(18)$$

In the original investigation, A was given without the $(\frac{4}{3})^{\frac{1}{2}}$ factor, which has the same origin as the $\frac{3}{4}$ factor in (8) and (10). The β factor, if any, has not yet been considered: it can scarcely exceed $\beta^{\frac{1}{2}}$ in k, but may be more serious in A. Provisionally the uncertainties of k and A may be taken as 1 in 800 and 1 in 100, respectively. In the course of a general revision and simplification of the theory I have not yet dealt with A, and it is therefore more provisional than the other constants.

For completeness it may be added that the theory (Eddington, 1940 a) gives for the mass of a neutron $\mathrm{M} + \frac{3}{2}\mu$, or very nearly $m_p + \frac{1}{2}m_e$. More tentatively

it gives $173{\cdot}8\ m_e$ for the mass of a mesotron, and $2{\cdot}36\ m_p$ for the mass of a " heavy mesotron " which decays into a proton or negatron. These results are not included in table 3.

§ 3. COMPARISON OF THEORY AND OBSERVATION

Setting aside the cosmological constants (R_0, N_0), the equations (1) to (14) boil down into a theoretical determination of three independent ratios $h\mathbf{c}/2\pi e^2$, m_p/m_e, $\kappa/\mathfrak{F}^2\mathbf{c}^2$. These, being purely numerical, do not involve the conversion constants, and therefore afford the most direct comparison of theory with observation. These are given in table 2 and compared with the results given by Birge, or obtained directly from his values of the constants.

The agreement seems not quite satisfactory, since the residuals of A and B are nearly twice their probable errors. Two such residuals out of three would be counted a significant discordance if they were independent. But the observed values of A and B both involve the observed constant $e/m_e\mathbf{c}$; and we find on examination that both residuals have the same source, namely a slightly discordant value of this constant. This is made evident by forming the combination $AB^{\frac{1}{2}}$, the observational value of which is independent of $e/m_e\mathbf{c}$.* The low probable

Table 2

Ratio	Calculated	Observed
$A=h\mathbf{c}/2\pi e^2$	137·000	$137{\cdot}030\pm{\cdot}016$
$B=m_p/m_e$	1837·46	$1836{\cdot}56\pm{\cdot}56$
$C=\kappa/\mathfrak{F}^2\mathbf{c}^2$	$8{\cdot}0939.10^{-37}$	$(8{\cdot}0981\pm{\cdot}0060)10^{-37}$
$AB^{\frac{1}{2}}$	1678·01	$1678.10\pm{\cdot}15$

error (1 part in 10 000) makes the test very stringent, and the agreement is perfect.

When the correlation of the residuals is allowed for, the agreement in table 2 is just about as close as the theory of probability admits.† But although the comparison does not indicate any real need for correcting the observed values, the theory insists that certain small corrections should be applied.

Birge's adopted value of $e/m_e\mathbf{c}$ is the mean of a deflection value and a spectroscopic value which are not very accordant. We shall find (§ 7) that the theory predicts a discordance of the two values with one another, and also a discordance with the value implied by the official definition of the other constants. Distinguishing the deflection and spectroscopic results (uncorrected) by the suffixes d and s, the theory in § 7 gives

$$(e/m_e\mathbf{c})_d=\beta^{-\frac{1}{24}}e/m_e\mathbf{c}, \qquad (e/m_e\mathbf{c})_s=\beta^{-\frac{1}{12}}e/m_e\mathbf{c}. \qquad \ldots\ldots(19)$$

Birge's values of $e/m_e\mathbf{c}$ when corrected in accordance with (19) become

spectroscopic $(1{\cdot}75987\pm{\cdot}00028)10^7$,
deflection $(1{\cdot}76013\pm{\cdot}00024)10^7$.

* It involves $\mathfrak{R}$, $\mathfrak{F}$ and the Avogadro number.

† It is an even chance that the worst of three residuals is less than 1·87 times its probable error.

Adopting the mean, $e/m_e\mathbf{c} = 1{\cdot}76000 \cdot 10^7$ instead of Birge's uncorrected mean (1·7592), we obtain for the ratios A and B in table 2 the revised comparison:

	Calculated	Observed
$h\mathbf{c}/2\pi e^2$	137·000	137·009
m_p/m_e	1837·46	1837·40

I do not venture to give the probable errors of the observed values, which require technical discussion; but, although they are not quite so small as the agreement suggests, they are considerably smaller than in table 2, owing to the removal of the disagreement between the spectroscopic and deflection methods.

§ 4. VALUES OF THE CONSTANTS

The calculated values of the most important constants are given in table 3. All except nos. 16, 17 are in C.G.S. units. The authority for the observed values is Birge's *Report*, except no. 17 (Hubble and Humason, 1931) and nos. 18, 20 (Thaxton, 1940). The probable error, given in the last column, is in units of the last decimal place in the preceding column. The observed values are taken directly from Birge and the other authorities without correction.

Table 3

Ref. no.	Constant	Description	Calculated	Observed	P.E.
1	e	elementary charge (e.s.u.)	$4{\cdot}80333 \cdot 10^{-10}$	4·8025	10
2	h	Planck constant	$6{\cdot}62504 \cdot 10^{-27}$	6·6242	24
3	M	mass of ^{1}H atom	$1{\cdot}67368 \cdot 10^{-24}$	1·67339	31
4	μ	$m_pm_e/(m_p+m_e)$	$9{\cdot}09873 \cdot 10^{-28}$	..	..
5	m_e	mass of electron	$9{\cdot}10368 \cdot 10^{-28}$	9·1066	32
6	m_p	mass of proton	$1{\cdot}67277 \cdot 10^{-24}$	1·67248	31
7	κ	constant of gravitation	$6{\cdot}66649 \cdot 10^{-8}$	6·670	5
8	$h\mathbf{c}/2\pi e^2$	fine-structure constant	137	137·030	16
9	h/e	. . . .	$1{\cdot}37926 \cdot 10^{-17}$	1·37933	23
10	m_p/m_e	mass-ratio	1837·46	1836·56	56
11	$e/m_e\mathbf{c}$	(deflection method)	$1{\cdot}76006 \cdot 10^7$	1·75959	24
12	$e/m_e\mathbf{c}$	(spectroscopic method)	$1{\cdot}76006 \cdot 10^7$	1·75880	28
13	N_0	particles in universe	$2{\cdot}36216 \cdot 10^{79}$	..	..
14	M_0	mass of universe	$1{\cdot}97675 \cdot 10^{55}$	..	..
15	R_0	Einstein radius of space	$9{\cdot}33544 \cdot 10^{26}$	..	..
16	R_0	(in megaparsecs)	302·38	..	..
17	V_0	nebular speed (km. sec.$^{-1}$ mp.$^{-1}$)	572·36	560	..
18	k	nuclear range-constant	$1{\cdot}92079 \cdot 10^{-13}$	1·9	..
19	A	nuclear energy-constant	$4{\cdot}2546 \cdot 10^{-5}$	..	..
20	$A/m_e\mathbf{c}^2$	. . . .	52·006	51·4	..

The main purpose of table 3 is to give explicitly the calculated values. With the exception of nos. 8, 10, 13, they involve the conversion constants in table 1; but the effect of any future changes in the adopted values of $\mathfrak{F}$, $\mathfrak{R}$, $\mathbf{c}$ is readily found from the physical dimensions of the constant considered, as explained in § 1.

Nos. 1 to 12 are all obtained by combining the three ratios in table 2 with the three conversion constants. Thus the comparison with Birge's values adds

nothing essentially new to the discussion in § 3: the same agreement and the same mild discordance with the uncorrected observed values is shown (with much redundancy) in another form.

Besides the accurate comparisons, the observational determinations of nebular recession and of the two nuclear constants provide independent but much rougher comparisons (nos. 17, 18, 20). It is worth remarking that by equations (16) and (18)

$$kV_0 = \mathbf{c}/\sqrt{(3N_0)}, \qquad (N_0 = \tfrac{3}{2} \times 136 \times 2^{256}), \qquad \ldots\ldots (20)$$

so that the range-constant of the proton-proton force can be found directly from the speed of recession of the galaxies, or vice versa, with no other observational data except the velocity of light.*

An improvement in the observational determination of κ is greatly to be desired. It has been suggested to me that the observed value should differ from my calculated value by an amount depending on the mean packing-fraction of the elements of which the universe is composed: but I find that the suggestion is fallacious. In the theoretical calculation the universe was represented as a uniform static zero-temperature distribution of protons and electrons. It is, of course, legitimate to rearrange the matter of the universe so as to simplify the theoretical determination, just as it is legitimate to rearrange the matter in the laboratory so as to simplify the observational determination: but in such rearrangement the experimenter cannot, and the theorist must not, violate the conservation of energy. There is therefore an implicit assumption that the energy of free radiation, cosmic rays, etc., in space is just sufficient to transmute the complex elements back into hydrogen, restore the gravitational energy lost in the formation of condensations, and leave a uniform distribution at zero temperature. For theoretical reasons I believe this to be true; but I should now describe it as a *consequence* of the formula for κ, and not an assumption that must be made in order to derive it. In formula (12) all reference to the "universe" is eliminated, and the ratio $\kappa/\eth^2\mathbf{c}^2$ of locally measured constants is expressed in terms of the number N of independent quadruple wave functions introduced by our system of analysis. The fact that the wave functions are non-integrable emphasizes that N (unlike N_0) is a local concept. We should ordinarily say that the remote environment has no effect on $\kappa/\eth^2\mathbf{c}^2$, because it has the same effect on $\eth^2\mathbf{c}^2$ as on κ.

§ 5. THE PROBLEM OF CONSISTENCY

Mass, momentum, energy, electric charge, etc., are primarily defined in molar physics. In some cases the molar definition can be adapted to cover microscopic quantities. Thus the mass M of a hydrogen atom is $1/n$ of the molar mass of a quantity of hydrogen composed of n atoms: and the mass of any *neutral* atom or particle can be defined similarly. The elementary charge e is $1/n$ of the molar electric charge of a body from which n electrons have been removed. Microscopic quantities defined in this way are said to be "molarly controlled". There is one important microscopic constant which is not molarly controlled, namely

* For an accurate determination, a correction must be applied to the observed nebular recession to obtain the limiting speed of recession. Ideally the correction can be found by astronomical determinations of the average density of matter in space, but these are at present too inaccurate.

m_e. The mass of an electron is obviously not defined as $1/n$ of the molarly measured mass of an aggregation of n electrons: the observer (if he escaped electrocution) would have no idea how to set about so unprecedented a measurement.

In quantum theory a fresh start is made, and the terms mass, energy, charge, etc., are applied to *analogues* of the molar quantities, which play a corresponding part in the equations of quantum dynamics to that played by the originals in classical dynamics. Whether the analogy is to be construed as identity (for such quantities as are molarly controlled) can only be ascertained when quantum theory is extended into a unified microscopic and molar theory as in *P. & E.* We must not assume the identity prematurely.

There is usually a definite understanding that, whatever other divergences may exist, lengths and periods in microscopic structures are molarly controlled. This is implied when we assume that the frequencies of light waves emitted by an atom are equal to the frequencies of oscillating sources in its structure; for the undulatory theory of light is part of molar theory. This assumption or convention was the origin of Heisenberg's theory, which superseded Bohr's theory of the atom; and it is too deeply rooted in wave mechanics to be disturbed. Molar control of lengths and periods is imposed through the formula (14) for the Rydberg constant.

We naturally try to find a set of physical constants which, besides satisfying molar control of lengths and periods, shall (as a minimum) be consistent with

(1) the analogy between quantum dynamics and molar dynamics;
(2) molar control of M and other neutral masses;
(3) molar control of e;

but it is by no means clear that even these minimum conditions can be satisfied simultaneously. Accepting (1), the measure-scale of mass can certainly be adjusted to satisfy (2); but it appears that a rather different scale is required to satisfy (3). This is the cause of the corrections introduced in equation (19).

For the theorist, it is impossible to sacrifice (1), and evidently (3) is the condition to be dropped. This means that the Faraday constant $\mathfrak{F} = e/\mathrm{MC}$ used in microscopic physics will not agree with the constant found by molar observation, and there will be corresponding differences in the constants e and h. This, however, is a small price to pay for the far-reaching developments of quantum mechanics which can only be utilized by accepting condition (1).

But, notwithstanding its advantages, I clearly cannot adopt this system of definition of the constants in the present context. To tamper with the Faraday constant would bar out all comparison with Birge's constants at the very outset. I must follow him in accepting conditions (2) and (3) as a *sine qua non*. This will lead to trouble sooner or later, when we try to utilize results predicted by quantum theory on the basis of condition (1). The trouble does not begin immediately, because we have a constant m_e which is not molarly controlled, and is (on this system) virtually defined so as to satisfy the most immediate requirements of condition (1). The formulae of § 2 and the calculated values in table 3 relate to this system of constants, which we shall call the " official system ". It will be treated in § 6. The " trouble " which arises in the adapta-

tion to the official system of results which are based on condition (1) will be treated in § 7.

To sum up:—We accept the ruling that in microscopic physics lengths, periods and the masses of electrically neutral objects must agree with the molar definitions.* As regards electric charges and the masses of charged particles, we have a choice of two evils. The quantities which appear in the equations of quantum theory are the analogues (not the direct equivalents) of molar charge and mass, and the theorist would naturally prefer that this should be the recognized definition of constants such as m_e and e. But in the official system of constants, e is defined as the direct equivalent of molar charge. (There is no direct molar equivalent of m_e). Thus the results of quantum theory normally require a correction to reduce them to the official system of constants. Although the formula for $\mathfrak{R}$ is a result of quantum theory, it is in an exceptional position because of the adoption of $\mathfrak{R}$ as a conversion constant. This requires that the quantity determined by it, namely $\mu c/h$, shall be the same in both systems of constants.

§ 6. THE β-COEFFICIENTS

The constant denoted by m_p/m_e is usually defined in the way indicated by the notation—a definition which would have had no meaning before the isolation of the electron. But the same constant can be described, independently of the atomic structure of electricity, as the ratio of the *densities* of the electrically positive and negative material in molar hydrogen. The related constant M/μ† can similarly be described as a density-ratio of external and internal energies in molar hydrogen. Relativity mechanics is based on the energy tensor, which includes the density as a component; so that in relativistic theory m_p/m_e and M/μ appear primarily as density-ratios, and their interpretation as mass-ratios comes later. By rearrangement of the theory in *P. & E.*, I now obtain equations (2) and (3), which yield $\mathrm{M}/\mu' = 136^2/10$, *before* treating microscopic problems.

In passing over into microscopic physics, we introduce the extra variate referred to in § 2, and this determines the scale of microscopic structure. Like all observables, it has an uncertainty: and its introduction involves an extra dimension of the domain of probability-distribution of the complete energy tensor, extending the number of degrees of freedom from 136 to 137. We find that this alters the measure of the energy tensor, and therefore of the density, in the ratio β; so that the density μ' is replaced in microscopic theory by the density $\mu_0 = \beta\mu'$. This is a distinction between the analogue μ_0 and the equivalent μ' of the molar density.

My earlier work in *P. & E.* did not go beyond this point; but it is clear that when we modify the definition of density to suit microscopic theory some further adjustment will become necessary. Either the constant of gravitation must be modified, or the metric of space will be changed. By accepting the condition of molar control of e, we settle the way in which this adjustment is to be made. For e^2 is of dimensions $\mathrm{ML}^3\mathrm{T}^{-2}$; and in order that its measure may be unaltered, the change of measure of density (dimensions ML^{-3}) in the ratio β must be

* This does not apply to constants or systems of measurement employed in auxiliary theoretical investigations (e.g. μ_0, $\mathfrak{R}'$ in § 6) which are eliminated in the final formulae.

† To six significant figures $\mathrm{M}/\mu = m_p/m_e + 2$.

coupled with a change of L^6T^{-2} in the ratio β^{-1}. To preserve relativistic symmetry L and T must be altered in the same ratio; hence the increase of density in the ratio β is coupled with a decrease of lengths in the ratio $\beta^{-\frac{1}{2}}$. It may be noticed that this combination makes Action invariant—a highly satisfactory property from the ordinary dynamical point of view.

The constant $\mathfrak{R}$ in table 1 is found by a molar measurement of light in its molar aspect (undulatory theory). Being the reciprocal of a length, it is transformed into a constant $\mathfrak{R}' = \beta^{\frac{1}{2}}\mathfrak{R}$ when μ' is transformed into $\mu_0 = \beta\mu'$. The relation found in the theory of the internal wave functions of a hydrogen atom is therefore primarily

$$\mathfrak{R}' = \frac{1}{2}\left(\frac{1}{137}\right)^2 \frac{\mu_0 \mathbf{c}}{h}, \qquad \ldots\ldots(21)$$

since h (which has the fixed relation (1) to e) is an invariant of the transformation. This gives

$$\mathfrak{R} = \frac{1}{2}\left(\frac{1}{137}\right)^2 \frac{\beta^{\frac{1}{2}}\mu' \mathbf{c}}{h}. \qquad \ldots\ldots(22)$$

Comparing with (14), we have $\mu = \beta^{\frac{1}{2}}\mu'$, which is the equation (6) that has been used in our calculations. We notice that molar control of e (and h) is secured by the choice of transformation; molar control of period and length is imposed by making (22) agree with (14); and molar control of mass is in this problem irrelevant, since we are not dealing with a neutral particle.

Equation (8) is made comparable with (21) by writing it in the form

$$R_0^{-1} = \text{numerical constant} \times \frac{m_0 \mathbf{c}}{h}.$$

The theory gives primarily a constant $R_0'^{-1} = \beta^{\frac{1}{2}} R_0^{-1}$, just as it gives $\mathfrak{R}' = \beta^{\frac{1}{2}}\mathfrak{R}$; so that the factor $\beta^{\frac{1}{2}}$ is introduced when we employ the ordinary Einstein radius R_0. There is no β factor in (9), which is a well known equation of molar relativity theory. The factor $\beta^{\frac{1}{2}}$ in (8) is carried through into (12), which is derived from (8), (9) and (13).*

This shows how the β-coefficients in the equations of § 2 have been determined, and we proceed to the explanation of the β-coefficients in equation (19).

§ 7. THE CONSTANT $e/m_e\mathbf{c}$

In a comprehensive treatment we should begin with the complete set of theoretical relations expressed in terms of the constants which the theory itself selects, and then, considering each kind of experimental measurement in turn, investigate the precise expression of the result of the measurement in terms of those constants. The existing treatment is an attempt at a short cut. Out of the wide variety of experimental measurements, three or four are selected on account of the great practical accuracy attained: constants directly related to these selected measurements are adopted, and the theorist is expected to transform his formulae so that they are expressed in terms of those constants. This

* Since the observed value of κ is relatively inaccurate, it is specially important to control the foregoing calculation of the β-coefficient in (12). I may therefore add that the simplest calculation employs the system of constants in § 7. The coefficient is then $\beta^{\frac{1}{2}}$; but the corresponding charge is e_β, so that $\mathfrak{F}$ must be replaced by $\mathfrak{F}_\beta$. By (28) we have $\beta^{\frac{1}{2}}\,\mathfrak{F}_\beta^2 = \beta^{\frac{1}{2}}\,\mathfrak{F}^2$, so that the formula is confirmed.

is a troublesome policy in the long run; for sooner or later one of the non-selected experimental measurements becomes the centre of attention. The special adaptation of the theory to the selected measurements becomes irrelevant, and we have to transform the formulae back to their original form before we can relate them to the new measurement. At present the kinds of measurement accurate enough to be used in determining the constants are very limited, and the trouble is localized: it is concerned with μ, and with m_e which is derived from μ.

A " spectroscopic " value of M/μ (or m_p/m_e) can be deduced from the ratio of the Rydberg constant for deuterium (or helium) to the Rydberg constant for hydrogen. Since this is a deduction of one purely numerical ratio from another, the theory is particularly simple; only we must use the primary theoretical formulae which satisfy condition (1) of § 5, and not the formulae adapted for purposes irrelevant to this particular measurement. The following remarks therefore relate to the primary theory.

To avoid unnecessary complication we employ " natural units " (Eddington, 1940) such that

$$\mathbf{c}=1, \qquad \kappa h^2=1. \qquad \ldots\ldots(23)$$

This leaves one disposable unit; and it is easily found that when the unit is changed so that densities are changed in the ratio β, masses are changed in the ratio $\beta^{\frac{1}{2}}$, and lengths in the ratio $\beta^{-\frac{1}{2}}$. Also h and e change in the ratios $\beta^{\frac{1}{2}}$, $\beta^{\frac{1}{2}}$. Proceeding as in § 6, we have in place of (22)

$$\mathfrak{R}=\frac{1}{2}\left(\frac{1}{137}\right)^2\frac{\mu'\mathbf{c}\beta^{\frac{1}{2}}}{h_s}, \qquad \ldots\ldots(24)$$

where h_s is the natural theoretical value of h in microscopic theory (the symbol h having already been appropriated to an adapted value). Comparing with (22), we have

$$h_s=\beta^{\frac{1}{12}}h. \qquad \ldots\ldots(25)$$

The difference is that h corresponds to conditions (2) and (3) in § 5, and h_s corresponds to conditions (1) and (2). Since e is always related to h by $hc/2\pi e^2=137$, we have $e_s=\beta^{\frac{1}{24}}e$. The value e is the equivalent of molar charge (i.e. it is molarly controlled), and the value e_s is the analogue of molar charge employed in quantum dynamics. The factor $\beta^{\frac{1}{12}}$ in (25) is the result of taking out the transformation of length in the ratio $\beta^{\frac{1}{2}}$ imposed by condition (3) before putting in the transformation in the ratio $\beta^{\frac{1}{2}}$ imposed by condition (1). Since condition (2) is common to the two systems, M is unchanged; and μ' is also unchanged, since the relation $\mu'=10\mathrm{M}/136^2$ is independent of microscopic theory as explained at the beginning of § 6.

The " observed " spectroscopic value of M/μ or m_p/m_e is computed on the assumption that the classical formulae (4) and (5) for transforming a two-particle system into external and internal particles apply. This assumption commits us to condition (1); for evidently a classical formula will not apply to quantities in quantum theory unless they are the analogues of the classical quantities. On

this understanding it is easily proved that the assumption is valid.* Thus the uncorrected spectroscopic constants $(\mathrm{M}/\mu)_s$ and $(m_p/m_e)_s$ correspond to the values h_s, e_s, and are inconsistent with the system of constants in table 3 which corresponds to h and e. In particular, the corresponding Faraday constant $\mathfrak{F}_s = e_s/\mathrm{M}\mathbf{c}$ is $\beta^{\frac{1}{24}}\mathfrak{F}$.

By (13), (14) and (1)

$$\mathfrak{F}^2\mathfrak{R} = \frac{1}{2}\left(\frac{1}{137}\right)^2 \frac{e^2}{h\mathbf{c}} \frac{\mu}{\mathrm{M}^2} = \frac{1}{4\pi}\left(\frac{1}{137}\right)^3 \frac{\mu}{\mathrm{M}^2}. \qquad \ldots\ldots(26)$$

Since $(\mathrm{M}/\mu)_s$ corresponds to $\mathfrak{F}_s$ instead of $\mathfrak{F}$, and the other constants are unchanged, we have

$$\frac{(\mathrm{M}/\mu)_s}{\mathrm{M}/\mu} = \frac{\mathfrak{F}^2}{\mathfrak{F}_s^{\,2}} = \beta^{-\frac{1}{12}}. \qquad \ldots\ldots(27)$$

and $(m_p/m_e)_s$ differs from m_p/m_e in nearly the same ratio. The collected comparison is

$$h_s = \beta^{\frac{1}{12}} h, \qquad \mu_s = \beta^{\frac{1}{12}} \mu, \qquad e_s = \beta^{\frac{1}{24}} e, \qquad \mathfrak{F}_s = \beta^{\frac{1}{24}} \mathfrak{F}. \qquad \ldots\ldots(28)$$

Applying these factors to the constants already calculated, we obtain

Table 4

$\mathfrak{F}_s = 9576{\cdot}48$	
$e_s = 4{\cdot}80480\,.\,10^{-10}$	
$h_s = 6{\cdot}62908\,.\,10^{-27}$	
$(m_p/m_e)_s = 1836{\cdot}34$	
$(e/m_e\mathbf{c})_s = 1{\cdot}75899\,.\,10^{7}$	(Birge $1{\cdot}75880 \pm {\cdot}00028$)
$(e/m_e\mathbf{c})_{ss} = 1{\cdot}75953\,.\,10^{7}$	(Birge $1{\cdot}75959 \pm {\cdot}00024$)

The last two quantities require explanation. Birge does not give the observational value of $(m_p/m_e)_s$ explicitly, but combines it with the ordinary Faraday constant so as to obtain "the spectroscopic value of $e/m_e\mathbf{c}$". This is a preposterously hybrid constant, since the e of the Faraday constant and the m_e of the spectroscopic result belong to different systems. However, we can calculate it for comparison with Birge. The result, denoted by $(e/m_e\mathbf{c})_s$, agrees excellently.

The *pure* spectroscopic value, which we denote by $(e/m_e\mathbf{c})_{ss}$, is obtained by combining $(m_p/m_e)_s$ with $\mathfrak{F}_s$. This is the value which we should expect to obtain by a measurement such as the deflection experiment which determines $e/m_e\mathbf{c}$ directly. This conclusion requires careful verification, since the comparison of a dimensional quantity with observed measurements is a more complicated matter than the comparison of a numerical ratio m_p/m_e. The scrutiny is as follows :—

The quantities actually measured in the deflection experiment are molar electromagnetic fields X, H and the curvature of track of a wave-packet. The curvature measurement applies directly to the theoretical equations, since lengths in those equations are molarly controlled. But it might be questioned

* Valid in so far as the only relevant difference between the proton and the deuteron is the difference of mass. I do not profess to have examined whether the effect of the complexity of the nucleus has been rigorously allowed for in the reductions.

whether X and H can be used as they stand, since we have seen that $\mathfrak{F}$ is inapplicable. Consider a modification of the experiment in which the fields are produced by charged bodies whose positions, velocities and molar charges ne are measured, so that n is determined from the molar measurement of charge by using the known constant e. The positions, velocities and n are transferred directly into the microscopic equations, and we can deduce from the experiment the pure spectroscopic constant $(e/m_e\mathbf{c})_{ss}$. It is clear that the two forms of experiment must give the same result, for a discrepancy would imply that it makes a difference whether we measure X and H or measure the molar charge and velocity which produce them; that is to say, it would be a discrepancy in Maxwell's theory. Thus X and H require no correction, the adjustment to microscopic theory being automatically provided for in the fact that they are computed (or measured) as though the body producing them had a charge ne instead of ne_s.

Since $(e/m_e\mathbf{c})_{ss}=(e/m_e\mathbf{c})_d$, we have inserted Birge's value of $(e/m_e\mathbf{c})_d$ for comparison in table 4. In § 3 the comparison was made the reverse way by applying corrections to Birge's values of $(e/m_e\mathbf{c})_s$ and $(e/m_e\mathbf{c})_d$ to obtain the official constant $e/m_e\mathbf{c}$.*

In table 2 (p. 120) of Birge's *Report*, twelve determinations of $e/m_e\mathbf{c}$ by seven distinct methods are collected, and the table conveniently indicates how the results depend on the Faraday constant. We have not scrutinized each method separately, but there seems no reason to suspect any theoretical error except the use of an inconsistent Faraday constant. A constant larger in the ratio $\mathfrak{F}_s/\mathfrak{F}$ or $\beta^{\frac{1}{24}}$ should have been employed. This correction should make all the determinations agree. The amended values correspond to $(e/m_e\mathbf{c})_d$, and a further multiplication by $\beta^{\frac{1}{24}}$ is necessary to reduce them to official reckoning.

§ 8. THE CONSTANT h/e

Another question of practical importance is whether the " direct value " of h/e should agree with the indirect value. According to Birge, the latest experiments give no indication of discrepancy; but in view of the stop-press character of this information, and the many vicissitudes of opinion, we desire an independent theoretical answer.

I assume that " direct " refers to a determination $h/e=\delta V/\mathbf{c}\delta\lambda^{-1}$, where $\delta\lambda^{-1}$ is the measured change of spectral wave-number due to the fall of an electron through a measured potential difference δV. We remark first that the equation does not introduce the constants μ, m_e, which are the root of the divergence of the theoretical and official systems. The theory of the experiment is in fact independent of the quantum dynamics of the atom, which only appears as a catalyst in the conversion of a molarly measured energy eV into a molarly measured energy $h\nu$. Also, if the experiment could be satisfactorily carried out with hydrogen, the whole theory of it would fall within the part of quantum theory where the official constants directly apply. In any of these ways we can see that the direct value will agree with the official indirect value in table 3. Alternatively

* The formulae (19) are not quite exact, owing to the difference of transformation of M/μ and m_p/m_e; but the inaccuracy does not affect the first six significant figures.

we can eliminate h by the fixed relation $hc/2\pi e^2 = 137$, and regard the experiment as a determination of e by the equation

$$e = \frac{1}{137}\frac{\delta V}{2\pi\delta\lambda^{-1}},$$

and it is clear that e will be in the same molar electrical measure in which V is expressed.

The theory therefore predicts agreement of the direct and indirect values.

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NOTE ON SOME RECENT PAPERS ON PHYSICAL QUANTITIES AND THEIR DIMENSIONS

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ABSTRACT. Examination of recent papers on dimensions and physical quantities suggests a need for more careful definition of these quantities. This matter was dealt with in an earlier paper, but is now expanded and treated in more detail. It is maintained that if definitions were more precise there would not be the same confusion about the dimensions of certain quantities. There may still be differences of opinion on the most appropriate definition of a particular quantity, but if the definition is adequate then there is no doubt about the dimensions of the quantity as deduced from the definition. In this connection there is discussed the use of quantities defined as ratios, such as specific gravity (or relative density), and their more fundamental equivalents, such as density. Further, a logical set of definitions leads to the conclusion that the introduction of certain universal constants is unnecessary—for example, the Newtonian constant N in the equation for force, $F = Nmf$. Finally, there is discussed briefly the definition of the intensive quantity, temperature, in terms of the extensive quantity, energy.

§1. INTRODUCTION

FROM consideration of the recent papers on dimensions and on the nature of physical quantities*, there are two points which emerge:

(*a*) That unnecessary quantities are introduced into physics, partly because of lack or ambiguity of definition, and

(*b*) That much of the discussion on the dimensions of physical quantities is misplaced in that the difference of opinion on dimensions derives from differences in the definition of the quantities, and the emphasis of much of the discussion

* Brown (1941), Yarnold (1942), Benham (1942) and Hansel (1942).

which takes place on dimensions should be moved a step further back to the definitions.

These two points are not entirely independent, but, to begin with, it will be convenient to treat the first apart from any dimensional considerations.

2. UNNECESSARY QUANTITIES IN PHYSICS

Let us consider the following four expressions

$$F = G\frac{M_1 M_2}{d^2}, \qquad \ldots\ldots(1)$$

$$F = NMf, \qquad \ldots\ldots(2)$$

$$F = \frac{a}{K}\frac{q_1 q_2}{d^2}, \qquad \ldots\ldots(3)$$

$$F = \frac{b}{\mu}\frac{m_1 m_2}{d^2}, \qquad \ldots\ldots(4)$$

where M, q, m, f, F are respectively mass, charge, magnetic pole, acceleration, force, and G, N, a, b are constants.

Benham (1942) insists on the necessity for retaining both G and N, while Yarnold (1942) requires that a and b should appear in the appropriate equations.

First let us consider equations (1) and (2). Assuming mass, length and time as indefinables, there are three quantities remaining which require definition, viz. F, G and N. As was pointed out in a previous paper (Duncanson, 1941), one equation (such as, for instance, equation (1)) cannot be used to define two quantities. Any equation must either express a relationship between known quantities, or define one quantity in terms of the other *known* quantities in the equation—or, more generally, the number of independent equations must equal the number of quantities to be defined. Now, unless F is otherwise defined, equation (1) contains two undefined quantities, F and G. The only other way of defining F is by using a method such as that expressed in equation (2) (or its equivalent in terms of momentum). If, then, we define force in terms of mass and acceleration, why is it necessary to introduce a further quantity N to confuse the issue? If an equation such as (2) is used to define force, then *force equals the product of mass and acceleration* can be taken as the definition of force, and there is no question of further quantities involved. To press the point further, if N is introduced into (2), why should not density be written as

$$\rho = C\frac{M}{V},$$

where C is a physical quantity with dimensions?

If it is still insisted that G and N must both be retained, then a definition of force must be given which is independent of both of them. Hansel (1942), for instance, states (p. 168): "Newton's first law of motion provides a definition of mass and a definition of force", and then, assuming that force has been adequately defined, proceeds to state Newton's second law (p. 175): "That the ratio of the force acting on a body to the acceleration which that force gives to the body is constant". Now Newton's first law certainly does not define the physical quantity, force. It introduces us to the concept of force, but there is no definition

of such a quantity in terms of other previously defined quantities, and, therefore, the second statement cannot be regarded as a law, but as a definition of force.

Let us now consider equation (3)—and remarks made about this equation will be equally applicable to (4). It is unnecessary to introduce both a and K into (3). Some quantity is needed to take care of the variation of force with the material of the dielectric, but Yarnold does not appear to justify the inclusion of a. If it were necessary to take into account some other variable quantity, then a would have to be retained. In this respect I am in agreement with Guggenheim (1942) when he suggests that two distinct quantities are, in practice, used in this connection, viz. permittivity, which we shall denote by k, and specific inductive capacity (or dielectric constant), denoted by K.

The logical procedure is to define the permittivity k by the equation $F=q_1q_2/kd^2$, and the specific inductive capacity K by $K=k/k_0$, where k_0 is the permittivity of a vacuum.

Similar pairs of quantities are used in the case of density and specific gravity, and of thermal capacity per unit mass and specific heat, where *specific* is used in each case to denote the ratio. It would be more satisfactory to use *relative* rather than *specific* to indicate the various ratios: for example, relative density, relative thermal capacity per unit mass, relative permittivity. But our chief concern here is not nomenclature but emphasis of the fact that two different quantities are often used for the same purpose.

This practice of expressing the magnitude of a physical quantity in terms of its magnitude for a particular substance under given conditions is quite common in physics, although it is not always as obvious as in the instances given immediately above. For example, the expression of resistance in terms of that of mercury under certain conditions; or again, the electric current in terms of the current necessary to deposit a certain mass of silver in a given time. These, together with specific inductive capacity, will be recognized as being on the same footing as specific gravity, which is, simply, the mass of a given volume of the substance in terms of the mass of the same volume of water.

The point it is desired to emphasize here is that these ratios must be recognized as distinct from the fundamental quantities concerned, and care should be taken when introducing them into equations, as the two types of quantity are not interchangeable.

§ 3. DIMENSIONS AND DEFINITIONS

Once we have defined our quantities with certainty, there is then no doubt about their dimensions, which brings us to our second point, namely, that any differences in the dimensions of a quantity result from differences in the definition, and that discussion should centre round the definition rather than the dimensions. A case in point is that of temperature. Benham (1942), Brown (1941) and Duncanson (1941) give $[L^{-1}]$, $[L^2T^{-2}]$, and $[ML^2T^{-2}]$ respectively for the dimensions of temperature. Three possibilities arise;

(*a*) That the respective definitions from which these dimensions are deduced define different quantities. The point at issue would then be which was the most desirable quantity to use from a practical and a theoretical point of view, a consideration we will not pursue further here.

(*b*) That some or all of the definitions are inadequate,

(*c*) That different dimensions are derived from equivalent definitions owing to the presence in the definition of quantities whose own dimensions are disputed. But this only throws the discussion a stage further back, to the definition of these quantities.

With these possibilities in mind, let us examine the three following definitions of temperature from which result the three sets of dimensions quoted above.

1. Brown defines temperature in the following manner. Firstly, specific heat is defined as a ratio, thus having no dimensions: then temperature is defined from the equation

$$\text{mass} \times \text{specific heat} \times \text{temperature} = \text{energy},$$

and from this the dimensions of temperature follow immediately as $[L^2T^{-2}]$.

2. The present writer does not believe that these two definitions can be used in conjunction with each other, and defines the temperature of a gas in terms of the mean energy of the molecules of that gas (actually 2/3 of the mean energy).* Hence it is seen that the dimensions of temperature are the same as those of energy, namely $[ML^2T^{-2}]$.

3. Benham introduces temperature with the statement: "We shall now take temperature as energy per unit mass, understandable from the kinetic theory and from calorimetry with heat as a form of energy". From this he deduces that the dimensions are those of $\left[\frac{\text{energy}}{\text{mass}}\right]$, which appears to be the same as those deduced by Brown, but on closer examination there emerge several distinct differences. Firstly, by the introduction of the Newtonian constant N, Benham deduces the dimensions of energy as $[ML^{-2}]$, and hence those of temperature as $[L^{-2}]$: this difference arises, not primarily from the definition of temperature, but from the dimensions ascribed to energy. Secondly, his statement of temperature as it stands is not at all clear. Assuming the normal concept of temperature is intended, then the statement is not true if it is meant that the temperature of a substance is the energy per unit mass of that substance; two different substances in thermal contact would then be at different temperatures. The alternatives are that Brown's definition is intended, or that the temperature is to be defined in terms of the energy per gram molecule of the substance—consistent with the dimensions of $\left[\frac{\text{energy}}{\text{mass}}\right]$ if, as is often assumed, gram molecule has the dimensions of [mass]. But it has already been pointed out in our previous paper that gram molecule is often used as a conventional term for denoting a given number of molecules, and, therefore, when used in this sense it is a pure number and has no dimensions. After all, in order to express the temperature of a substance in terms of the energy per gram molecule we have to select a different mass for each substance—for example, 2 gm. of hydrogen, 20 gm. of methane. To say that this results in $\left[\frac{\text{energy}}{\text{mass}}\right]$ for the dimensions of temper-

* A more general statement is also given to cover any substance, as well as radiation (Duncanson, 1941).

ature is equivalent to deducing the dimensions of intensity of magnetization to be $\left[\frac{\text{magnetic moment}}{\text{mass}}\right]$ because for iron it is the magnetic moment of 7·8 gm. or for mercury the magnetic moment of 13·6 gm. It is obvious that these masses of iron and mercury have been chosen in order to obtain unit volume (1 c.c.) of each substance, so that this is merely another way of defining intensity of magnetization as magnetic moment per unit volume. Similarly, the masses of hydrogen and methane have been chosen to obtain the same definite number of molecules in each case, so that temperature defined in terms of energy per gram molecule has the dimensions of [energy]. The fact that the particular number of molecules is not known exactly is immaterial as long as there is a means of selecting the same number for each different substance.

Returning to Benham's deduction that temperature has the dimensions $[L^{-1}]$, we see that this will not hold if our previous contention is admitted that it is an unnecessary complication to introduce the constant N into the force equation. Moreover, not only is it a complication but, as we have already seen, it introduces two unknowns, namely F and N, into the one equation. At first sight it appears that Benham has devised an independent method for determining the dimensions of N. However, on examination it is found that, even after making several assumptions, he adopts a procedure which in the end amounts to making an arbitrary choice for the dimensions of N. For example, one of the alternative possibilities for its dimensions is rejected because the dimensions of temperature deduced therefrom, namely $[ML^{-1}]$, appear to have "no very obvious physical significance". Such a procedure does not seem to be sufficient to justify the introduction of N.

This matter has been dealt with in some detail as it demonstrates the importance of adequate definition of physical quantities, and the unnecessary confusion of dimensions that results unless this precaution is carefully observed.

§4. INTENSIVE AND EXTENSIVE MAGNITUDES

Tolman (1917) has pointed out that temperature is a quantity of intensive magnitude, while energy is of extensive magnitude. This may be raised as an objection to our definition of temperature in terms of the mean energy of a molecule. Let us recall that a quantity of extensive magnitude is such that if it has a value a_1 for body 1, and a_2 for body 2, then it will have a value a_1+a_2 when the bodies are taken together. Intensive quantities are not additive in this manner.

It is interesting to examine a little more closely the nature of some of these intensive quantities. It will be found that intensive quantities include some which are defined in terms of certain amounts * (it may be a certain number, length or mass, or, what comes to the same thing, some kind of average) of the matter to which the quantity refers, and it is easy to see then how these quantities cannot be extensive, as their very definition precludes this possibility. For example, the density of a substance is defined as the mass of a certain volume of that substance, and even if two bodies of this substance are added together,

* "Amount" is used rather than "quantity" to avoid using the latter with two distinct meanings.

their density remains the same, as it is still the mass of the same sample volume. It is easy to see that the same applies to such quantities as thermal capacity per unit mass and intensity of magnetization (magnetic moment per unit volume). Temperature is such a quantity. It is defined in terms of the extensive quantity, energy, but is prevented from being extensive itself by the fact that the energy is to be that of a stated number of molecules, or their mean energy, according to the definition preferred. So there is no inconsistency in temperature, which is an intensive quantity, having the same dimensions as that of the extensive quantity, energy.

NOTE ADDED IN PROOF. Since this paper was communicated, the author has had the opportunity of seeing two papers which cover, to some degree, similar ground to the above note, one by Dingle (*Phil. Mag.* **33**, 321, 1942) and the other by Guggenheim (*Phil. Mag.*, in the press).

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DISCUSSION

On papers by C. W. HANSEL (*Proc. Phys. Soc.* **54**, 151, 159, 164 (1942)).

Sir CYRIL ASHFORD. I am on Mr. Hansel's side in preferring $P=ma$ to $P/W=a/g$ as a teaching method—though that question seems to me of less importance than the fundamental one of whether school kinetics should be treated as a deductive or an inductive science. My experience goes to show that it can be plausibly presented in the latter way even to post-certificate schoolboys, but I rather think that it is only the pick of them that really appreciate the logical difficulties.

Mr. J. L. BRERETON. My position can be briefly summarized as follows :—

(1) I am not in agreement with the report of the Mathematical Association on the Teaching of Mechanics when it states that definition of mass (through inertia) should be avoided in the early stages by use of the formula $P/W=a/g$, because, from discussions with university teachers, I believe many pupils fail to change over effectively later to the more significant formula $P=ma$.

(2) Further, we have in secondary schools not only to train children to become future teachers or research workers, but also to see that those concepts which have helped to build our industrial world—mass, force, velocity, energy, temperature, etc.—become part of the ordinary awareness of the people. An example is the crying need, in relation to flying, for a thorough understanding of relative velocity, a conception which we have notoriously failed to inculcate. One of the most important of these concepts, at any rate for the chemist, and in many commercial transactions, is "quantity of matter" expressed in units of mass and measured, as Mr. Hansel puts it, in "any convenient manner". I cannot see that either mass or relative velocity is more difficult than the relation between heat and temperature, which most children tackle early in their school course and have little difficulty in grasping.

(3) I do not consider that a case has been made against the definition of mass as quantity of matter. For bodies of the same material, quantities of matter can be compared by means of volume or by chemical equivalence (as in volumetric analysis). For bodies of different material, quantities of matter can be compared by means of (i) weight, (ii) inertia. (*See* Clerk Maxwell, *Matter* and *Motion*, Art. 46.)

(4) This idea of using one quantity as a measure of another is important in itself. For example, in a car running at constant speed, distance travelled can be measured on a watch ; children learn to compare areas of cardboard by weighing, pressures by heights of a column of mercury, temperature by volume of gas, force by extension or twisting of springs, etc.

(5) My only quarrel with Mr. Hansel is in his generalization that a beam balance measures mass and a spring balance measures weight. Both instruments have to be used to measure both quantities, and the student must understand this. I cannot agree with his " mass in water " in an experiment on the principle of Archimedes. Surely the child can understand that *in certain circumstances* mass is proportional to weight and weight to mass.

(6) The objection is often raised that it is difficult to explain in simple language what one means by quantity of matter. But this applies equally to most physical quantities. Length, time, force can only be explained and defined in terms of the method of comparing two lengths, or two times, or two forces, as the case may be.

(7) Then there is the difficulty that the quantity of matter in (say) an electron varies with its speed ; but so, after all, does the length of a rod and an interval measured on a clock. It must be admitted that at present we are not in a position to introduce the ideas of the special theory of relativity in elementary mechanics classes.

(8) The fact that inertial mass and gravitational mass (*a*) are both additive, (*b*) are identical to the limits of our measurement, is strong justification for saying that mass is more fundamental than either of these manifestations, and that Newton's identification of it with quantity of matter was correct. After all there is no other property of bodies which is additive in the way in which mass is.

(9) I suspect that much of this attempt to soft-pedal the association of mass with quantity of matter is traceable to Ernst Mach, who was one of the first to give the definition of mass which assumes that the weight of a body is proportional to its inertia, but who had to discredit the concept quantity of matter in order to support his (to my mind untenable) theory that matter was nothing but a sensation in his brain. It is significant that in his scientific writings he was forced repeatedly to throw this idealist theory overboard.

Mr. K. J. Carpenter. In his paper on the definition of mass, Mr. Hansel glosses over the fact that gravitational force on a body, in a field of given strength, is in a constant proportion to the mass, where mass is found by comparison of inertia with a standard body (this definition of mass being independent even of the existence of such a thing as gravity).

This is a very remarkable phenomenon, and no more self-evident than to suppose the electrostatic and magnetic pole strengths of a body to be determined by its mass.

I therefore submit that the beam balance does *not* measure mass, but compares weights, because the opposing couples about the fulcrum are due to gravitational forces, which are present owing to the *weights* of the bodies.

In an article on physics fundamentals, could not some experimental evidence of the proportionality of mass and weight be included ?

The period of a compound pendulum may be written $\sqrt{\frac{Mk^2}{Wl}}$ (where M is its mass and W its weight). If, at a given place, the M/W ratio is always found constant, the proportionality will have been *approximately* checked.

Prof. J. A. Crowther. Though it is hardly possible to add materially to the very full and, I may add, extremely able discussion of the teaching of mass and weight contained in the paper by Mr. C. W. Hansel, I should like to take the opportunity of expressing

my agreement with his main conclusions. The chaotic state into which the teaching of mechanics has been thrown by well-meant, but misguided, attempts to placate the "practical man" following on the B.A. Report of 1907 is well known to all examiners of School Certificate physics. The mental confusion of these unfortunate candidates is as pitiable as it is unnecessary.

The trouble arises from two main causes. In the first place there has been a tendency to regard mechanics as a subject in itself, divorced from the great body of physical science of which it forms a small, though fundamental, part. Any system which necessitates such statements as "the mechanical equivalent of heat is 772 ft.lbs. per B.T.U. at Manchester" is automatically ruled out of court as a suitable medium for scientific thinking. Similar considerations apply to the suggestion, which Mr. Hansel appears to accept, that the gravitational system of units should be rationalized by the introduction of a formal unit of mass, the "Slug". A suggestion which would appear to demand such statements as "unit mass (1 slug) of hydrogen contains $2{\cdot}15\times10^{22}$ molecules at Wigan, and a sixth of that number on the surface of the moon", is not calculated to appeal to any serious scientific thinker.

That such a system should have found its way into our schools is due, I believe, partly to the confusion existing in so many quarters between education and technical training, and partly to the belief, often strongly held, though not invariably expressed, that if the two are distinct, the latter is fundamentally important; the former being merely a luxury, like whisky or cigars, not positively harmful unless indulged in to excess. Hence the stress on copying the methods of the "practical man". Education should precede, not accompany, technical training; and a properly educated student of mechanics will have no difficulty in realizing (will, in fact, have already realized) that where high accuracy is not important, the weight of a pound mass is a handy practical equivalent to a force of 32 poundals. If he finds that the eviscerated equation $P/W=a/g$ simplifies his calculations (which is doubtful) he will at least use it with understanding. I believe that gravitational units should be eliminated from our elementary teaching, or at any rate relegated to an appendix at the end of the text-book. No other single reform would do more to increase the efficiency of scientific education in our schools and universities.

Mr. A. F. Dufton. In his paper on Mass, Mr. Hansel is unrestrained in his criticisms of those who in the past have given consideration to the teaching of elementary mechanics: I note in particular his remarks about the report of a most competent and distinguished committee of the British Association. Clause 19 of this report he stigmatizes as an ill-conceived suggestion, and, apropos of Clause 20, he asks "Can anything be more fatuous?"

This question deserves an answer, and it will be found, I think, in § 9 of Mr. Hansel's paper, where, in an example of the Principle of Archimedes, the mass of a solid in air is 100 grams and the mass of the same solid in water is blandly stated to be 80 grams. Is not this the quintessence of fatuity? Where are the other 20 grams?

Would not all the pother about gravitational units disappear if the pound weight were used in conjunction with the pound mass and g were recognized as the corresponding unit of acceleration?

1 *lb.* weight produces 1 g acceleration, in mass 1 *lb.*

The gravitational system is a local system, and it is not the mass and the density of a body which vary from place to place but g, the unit of acceleration.

Mr. D. G. A. Dyson. I do not think that mass is a very easy idea to grasp really accurately, but neither is any fundamental concept; I certainly should not care to *explain* the quantities quoted as more difficult—length and time. Was Mr. Lauwerys very helpful in quoting from a number of distinguished men of various nationalities to show how difficult they found the clear understanding of mass? In any case, can the schoolboy be expected to have quite the clarity and accuracy demanded by scientific giants in their treatment of such fundamentals? I think the idea of inertia gives the meaning of mass quite as clearly as is necessary, especially after reference to trolley experiments and everyday experience. In any case, the idea of mass must be used sooner or later, and it is no solution to say that no one really understands it.

As far as $P/W=a/g$ is concerned, I do not understand why it was ever introduced. In the first place, I do not agree with Mr. Ghey's implication that a boy finds ratios easy to understand; my experience has been that this is one of the most difficult of elementary ideas for him to grasp. In the second place, the experimental demonstration of Newton's second law and its application to the definition of force units is perfectly easy to understand. I suppose ratios come into this derivation too, but not so fundamentally as in the P/W formula. In the third place, this P/W formula appears to tie everything to the idea of weight, whereas the $F=ma$ of Newton's second law is of universal application, and, once grasped, absolutely foolproof. I have felt for a long time that many of the new teaching methods designed to make the work easier for the pupil only do so (if at all) by evading the really important points.

I am not sure that I would go the whole way with Mr. Hansel in not using the gram weight, etc., in the early stages of mechanics, before the introduction of the laws of motion. I do not feel that their subsequent dismissal is very serious. On the other hand, I am bound to admit that his view-point is really sounder, and I am quite open to conviction. It is just whether one can be really strictly logical with children, whose minds are not able to grasp the whole range of the subject at once.

I was very sorry that the author did not have more support in the discussion, but no doubt there must have been many who did not speak, simply because they were in agreement with him.

Mr. G. W. E. Ghey. "A resultant force of 10 tons weight acts on a mass of 20 tons. What is the acceleration"? Surely the simplest solution is the application of $P/W=a/g$, as follows: Since 20 tons weight acting on 20 tons gives acceleration g, therefore 10 tons weight acting on 20 tons gives acceleration $\frac{1}{2}g$.

Mr. G. F. Herrenden Harker. The most illuminating physical approach to Ohm's law is from energy considerations.

Consider a portion AB of an electric circuit consisting of a stationary metallic conductor of resistance r_{AB}. Then if V_A denotes the potential of A, V_B that of B, and if this potential difference V_A-V_B volts maintains a current of i amp. between A and B during an interval of t sec., thus transferring a quantity q $(=it)$ coulombs of electricity from A to B, the electrical energy expended between A and B will be given, in joules, by $w_{AB}=(V_A-V_B)q$.

If, further, the only physical phenomenon occurring between the points A and B is an evolution of heat within the conductor, the heat energy liberated will, by Joule's law, be given by $w_{AB}=r_{AB}i^2t$, in which, if w_{AB}, i and t are measured in the units stated above, r_{AB} will be given in ohms.

Equating these two expressions for w_{AB} gives

$$V_A-V_B=r_{AB}i.$$

This is Ohm's law, which is accorded such exaggerated prominence in elementary texts. *It is only valid for a portion of an electric circuit in which the sole physical happening consists in an integral conversion of electrical energy into heat energy.* It thus provides the key to the solution of problems concerned with electric lamps, radiators, irons and toasters, which, however, by no means cover the entire or even the most important among the practical applications of electricity.

The portion of the circuit between A and B may include an apparatus capable of transforming some part of the electrical energy supplied into something other than heat. Thus, if the apparatus is an electric motor, some of the electrical energy supplied to it will appear in the form of mechanical work. If, of the total quantity W_{AB} of electrical energy supplied during a time t between the terminals A and B, a portion, w_{AB}, is transformed into heat, while the remainder, w'_{AB}, is converted into other forms, so that $W_{AB}=w_{AB}+w'_{AB}$, then, as before,

$$(V'_A-V'_B)i't=r'_{AB}i'^2t+w'_{AB},$$

i.e.,

$$V'_A-V'_B=r'_{AB}i'+w'_{AB}/q'.$$

The simple version of Ohm's law is thus no longer valid, and the second term on the right-hand side required to complete and generalize it, representing, as it does, the energy converted by the apparatus into forms other than heat per coulomb of electrical energy supplied, is normally of much greater importance than the first term on the right-hand side, representing the internal ohmic voltage drop within the apparatus. This second term on the right-hand side, obtained by dividing a number of joules by a number of coulombs, will be expressed in volts. The corresponding voltage $v'=w'_{AB}/q'$ can be regarded as acting in opposition to the applied voltage, so that, in formal analogy with the previous relation, we can write

$$(V'_A-V'_B)-v'=r'_{AB}i'.$$

Note that if the resistance between A and B remains unchanged, i.e. if $r'_{AB}=r_{AB}$ (as, for example, if the " conductor " considered in the first case is an electric motor with its shaft clamped, and in the second case is the same motor with its shaft free) and if the P.D. applied between the terminals A and B is the same in both cases, the current i' in the second case will be less than the current i in the first case. Alternatively, if the current is the same in both cases, the P.D. $V'_A-V'_B$ in the second case will be greater than the P.D. V_A-V_B in the first case.

When a generator of e.m.f. E volts maintains a current of I amp. in a circuit during an interval of t sec., the total electrical energy supplied by the generator will be given, in joules, by

$$W=EIt,$$

and will reappear in various other manifestations of energy throughout the entire circuit. Thus, if R denotes the resistance of the generator, r that of the remainder of the circuit, and if the whole of the electrical energy supplied by the generator suffers conversion into heat, then

$$EIt=(R+r)I^2t,$$

i.e.,
$$E=(R+r)I.$$

Note that as $v=rI=E-RI$, the P.D. between the terminals of the generator when furnishing current is less than its e.m.f. by the internal ohmic voltage drop within the generator itself.

If part of the electrical energy made available by the generator appears in some form of energy other than heat, and if, during the interval t, its amount is denoted by w', then

$$EI't=(R+r)I'^2t+w',$$

i.e.,
$$E=(R+r)I'+w'/I't.$$

The second term on the right-hand side having the physical dimensions of a voltage can be denoted by V', so that we can write, in formal analogy with the previous relation,

$$E-V'=(R+r)I'.$$

With regard to the enunciation of Archimedes' principle, I should like to urge the complete suppression of any mention of " apparent loss of weight " and the substitution of " upthrust " in its place.

Mr. J. A. Lauwerys. Mr. Hansel expresses the views of the school of thought described in the Mathematical Association *Report* as " being dominant at the end of the last century but now gone out of fashion ". Since I take it that Mr. Hansel wishes to convince us of the soundness of his views, he did well to start as he did. For in the three points which he asks us to assume, he has covered up in ambiguity and obscurity all the difficulties of the topic in such a way that, having once gone wrong there, one can never get straight again. In consequence one may be misled into accepting his conclusions.

(1) Take his first point. " It is advisable to teach a student the conception of mass and not to ignore it completely." What is meant by " teach the conception of mass " ? Give him some sort of an idea ? Or teach him enough to enable him to solve difficult problems—say about water-carts with holes in their bottoms running down a hill ? Are " teach " and " ignore " the only alternatives ? Lastly—most important—does the author here mean " inertia " when he says " mass " ?

(2) If we grant that mass, defined as quantity of matter, is a measure of inertia, the question might arise as to when the word is first to be introduced. But if we use $P/f=W/g$, we do not have to change over to any new system. At the appropriate moment, when the pupil has reached the appropriate stage of intellectual development—say at Higher School Certificate stage—we extend. As Prof. Alfred Lodge wrote in the *Math. Gazette* (Feb. 1938): "For beginners I am convinced that Newton's second law is best given as $P/W=f/g$, i.e. accelerations are proportional to the forces acting on a given body, actually or potentially. The equality $P/W=f/g=$a constant for a given body is more abstruse and should be delayed a bit. $W/g=m$ is chiefly a matter of *quality*. W and g are both vertical vectors; m is a scalar quite directionless. Important, but too advanced for youngsters: that is the reason for delaying a dissertation on M."

Incidentally, in the same number of the *Math. Gazette*, is printed a presidential address by the late Prof. L. N. G. Filon on this very topic; it concludes as follows:—"It seems at any rate remarkable that after all the fuss we have made about the fundamental distinction between mass and weight and the criticisms levelled at the engineer for his systematic use of W/g, it should turn out that the engineer was right after all, and that incidentally Newton's amazing intuition had, in that disregarded definition, hit upon the inevitable truth."

(3) What are these "unnecessary, unwanted, irrational units"? The weight gram or the pound weight?; the cm.?; the foot? The whole point of $P/W=f/g$ is that it does not bring in new units.

(4) Mr. Hansel's remark about the confusion in the minds of students in this country and in the U.S.A. has point only if the confusion does not exist in other countries. Let us then turn to Karl Hahn's monumental *Methodik des Physikalischen Unterrichts*. He spends page after page (4+18 quarto) deploring how hard it is to make pupils get their sums right, how muddled they are, how difficult the whole thing is, and his solution is: in the lower forms, by all means, try to make clear the idea of *Inertia* and distinguish it from *Weight*. After the age of 16, approach gradually the concept of mass—experimentally, e.g., via Attwood's machine, etc.

Henri Poincaré also spends a lot of time on this question. *Inter alia*, he asks how mass is to be defined, discusses it at length and concludes: "Mass is a numerical coefficient which mathematicians find it convenient to use in their equations." He deplores the bad teaching of mechanics in France. Pupils, he says, think of forces as arrows with which one draws parallelograms. How he wishes that it was taught as well as in England! There they teach it sensibly—as an experimental science: "Naturellement, les Anglais, comme toujours, ont raison."

(5) "A beginner can understand mass with less difficulty than he can understand length and time."

"Teach the distinction between mass and weight early in the science course."

These questions have been discussed at great length by many eminent mathematicians and logicians, e.g. by Ernst Mach in *The Science of Mechanics*, Karl Pearson in *The Grammar of Science*, Henri Poincaré in *Science et Methode* and *La Science et l'Hypothèse*. (See also Prof. Watson's *On understanding Physics* and Prof. Filon's address in *Math. Gazette*, Feb. 1938.)

I wish I could describe in detail what their conclusions are, but they all agree

(*a*) how very difficult the topic is, and how muddling to beginners;

(*b*) how Newton's genius shines out in his treatment of the question;

(*c*) how nearly impossible it is to give a satisfactory definition or explanation of what is meant by mass;

(*d*) that the only satisfactory approach is via Newton's laws of motion.

Thus, Karl Pearson says: "I must now ask the reader to follow me in a rather more difficult investigation .... $m_1f_1=m_2f_2$, etc. Mass accordingly as the ratio of the numbers of units in two accelerations is a conception which can easily be appreciated. It is in this manner that mass is invariably determined scientifically. Yet the reader will frequently

find mass defined as ' the quantity of matter in a body '. After our discussion the reader will easily appreciate how idle is a definition of mass in terms of matter ".

Again, from E. Mach :—" The expression ' quantity of matter ' is not adapted to explain and elucidate the concept of mass, since that expression itself is not possessed of the requisite clearness. It did not occur to Galileo that mass and weight were different things Huyghens, too, in all his considerations put weights for masses. . . . Physicists were not led to form the concept *mass* till they made the discovery that the *same* body can by the action of gravity receive different accelerations."

Then we have L. N. G. Filon :—" Newton, after talking round the question, defines mass by weight. Curiously enough, this reference to weight in the *Principia* seems to have been almost completely ignored, and for over two centuries we find the purely verbal definition of mass by ' quantity of matter ' repeated without explanation. If now we proceed in logical manner, and leave weight out of consideration for the present, ' mass ' in the second law is merely an undetermined constant belonging to a body, and it appears that all that the second law can tell us is how to compare forces."

Finally, H. Poincaré :—" The definitions proposed are sheer metaphysics."

(6) It is to be noted that the force equation $P/W=f/g$ has been proposed precisely by those who have thought carefully about the difficulties in the concepts and have been struck by the difficulties which the pupils meet when attempts are made to teach the distinction between mass and weight early in the course.

What they have wished to avoid is what the adoption of Mr. Hansel's ideas would ensure, viz., giving pupils the illusion of knowledge or substituting confusion for ignorance.

The best course to adopt is to follow the advice given in the Mathematical Association *Report*, thus making co-operation easier between teachers of science and of mathematics.

This means : (*a*) being careful to use words properly ourselves, as Mr. Hansel says, and (*b*) making clear at some stage the distinction* between inertia and weight, but not defining mass ; (*c*) leaving mass and absolute units until after school certificate.

(7) (*a*) For the rest, I would be quite willing to adopt the word *slug*, though as Mr. C. O. Tuckey says plaintively in a letter published in the *Math. Gazette* (1938, p. 282), " the name ' slug ' sounds idiotic and unscientific ". (Why do text-book writers hold the slug in contempt ? I believe the answer to be simple, though silly.)

(*b*) The $P/W=a/g$ formula has been preferred not only by B.A. Committees, but by most committees.

(*c*) Lastly : Do not imagine that ordinary weighing machines compare *masses*. They do not. They compare *weights*. Consider what would happen if a balance was placed in a highly heterogeneous gravitational field. We can use them to compare masses only after we have experimentally demonstrated Newton's law that $m \propto W$ (the foundations of Einstein's later work on General Relativity).

There is always a danger of treating dynamics chiefly as a mathematical discipline enabling the learners to solve formal problems and exercises ; as A. N. Whitehead says : "My own criticism of our traditional educational methods is that they are for too occupied with intellectual analysis, and with the acquirement of formularized information. We are too exclusively bookish in our scholastic routine. The general training should aim at eliciting our concrete apprehensions and should satisfy the itch of youth to be doing something. In the Garden of Eden, Adam saw the animals before he named them : in the traditional system, children named the animals before they saw them."

My own view of the teaching of dynamics is that it is a study of the real world, an attempt to explain it by generalizations which increase in scope as the experience of the learners grows, and where the rationalizations must increase in abstractness only as their intellectual grasp deepens. Hasten the process unduly and you help to build up an academic, scholastic mind, not a scientific one.

Mr. ROTTENBURG. To be inconsistent in teaching is always a great mistake, and we have been inconsistent when we have used the word *mass* for a property of a body in discussing linear motion, and the word *inertia* for the same property when discussing

* How has the confusion arisen in this paper ? The key is found in Mr. Hansel's sentence, " Mass is a property of things ever present in daily experience "; he means inertia, of course.

rotational motion. Newton was quite clear in teaching that *mass* was compounded of *volume* and *density*, i.e. that it was quantity of matter. He also describes the force a body exerts when it has its motion altered as the *vis inertiae*—in other words, that the inherent property that sets up the force is *inertia*. But even without Newton the use of two different words in the two cases remains a mistake. Mr. Hansel says we must teach "mass", clearly—but what happens then? The pupil goes out after the lesson and meets the word right and left—(the masses; the mass of the electorate; masses of this or that; mass production; mass psychology)—but used in a sense that is never what he has been taught. And the reiterated meaning (namely "a quantity of") will surely win. Every pupil, too, is liable to be struck by the fact that, even in his teaching, the inconsistency of using *inertia* in one set of cases and *mass* in the other is wrong.

When I first started to use the word *inertia* in teaching I found many pupils relieved when I informed them that it was the same property as had been taught them as *mass*. I also found that the word *inertia* helped me to keep my own ideas clear. One can then go on and explain that either *weight* or *inertia* can be used to measure Newtonian mass. If a man sits on the edge of a merry-go-round at rest with a balance and "weights" or bits of metal marked 1 lb., he can measure a mass or quantity of matter by balancing the bits of metal against the "mass" in the other pan. If he now starts up the merry-go-round, and it reaches a speed where the scale-pans fly out almost horizontally, he can still compare and balance the bits of metal and the "mass" but he will now be using their inertia to compare the masses, whereas, while at rest, he compared them by their weights.

Prof. G. Stead. As regards paper I, on Ohm's law, I am in general agreement with the conclusions set out on page 159, except that it seems to me to be too late in the day to make the changes in the meaning of *ampere*, *volt* and *ohm* suggested in § 5. Such changes would result in hopeless confusion and would do more harm than good.

I know from experience that the experimental illustration of Ohm's law described on pages 156–7 does, in fact, help beginners to grasp the essential point of the law, but I think that teachers should be perfectly honest with their pupils and tell them plainly that the experiment is in no sense a *proof* of Ohm's law. The measurements of P.D. are made with a moving-coil voltmeter, which itself depends on the truth of Ohm's law, and, therefore, all the experiment really proves is that the instrument maker has calibrated his meter with reasonable accuracy. The experiment should really be done with an electrostatic voltmeter, but this, of course, is impracticable, at least for beginners.

The extracts, in the paper on specific heat, from the writings of various physicists, seem to indicate that the whole question of the definitions of thermal quantities needs clarifying. It seems to me that it is not so much a matter of choosing between two alternative definitions of specific heat as of recognizing that nearly all of us are guilty of using the single term *specific heat* to denote two essentially different things. It is usually said that the water equivalent of a calorimeter of mass m grams and specific heat s is (approximately) ms gm., and that the thermal capacity of a body of mass m gm. and specific heat s is ms calories per degree. Clearly the first s is a pure number and the second is a dimensional quantity measured in calories per gram per °C. Hansel is, of course, correct in saying that, in the equation $H=ms(t_2-t_1)$, s cannot be a pure number, but he seems to have overlooked the fact that it is not this s which is measured either by the method of mixtures or by the method of cooling. If s is a dimensional quantity, the usual equation for the method of mixtures, viz.,

$$ms(t_2-t_1)=m_0(t_1-t_0),$$

is clearly wrong dimensionally, the correct equation being

$$ms(t_2-t_1)=m_0s_0(t_1-t_0),$$

where s and s_0 are the thermal capacities per unit mass of the solid and the water respectively and from this we find

$$s/s_0=m_0(t_1-t_0)/m(t_2-t_1).$$

Similarly, with the method of cooling, we compare a liquid with water and obtain

$$ms(d\theta/dt)=m_0s_0(d\theta/dt)_0,$$

which again gives us s/s_0 and not s. Thus, the elementary methods of determining specific heat give the result as a ratio. If anyone doubts this, I would point out that in mixture and cooling experiments both masses and temperatures can be expressed in any units whatever without affecting the result.

Electrical methods give, in the first place, a measurement of the thermal capacity per unit mass expressed in ergs (or joules) per unit mass per degree, and a conversion of the result to calories per gram per °c. involves a determination of J. If we also do this electrically, we have in effect determined the thermal capacity per unit mass (a) of the given substance, (b) of water, and might well write

$$Jms\,(\theta'-\theta) = Eit,$$

$$Jm_0s_0(\theta_0'-\theta_0) = E_0i_0t_0,$$

so that

$$s/s_0 = Eit(\theta_0'-\theta_0)m_0/E_0i_0t_0(\theta'-\theta)m.$$

Thus we obtain s/s_0 as a pure number, and do not require to know the value of J.

It seems to me to be a matter of little importance whether the term *specific heat* is used to express a ratio or a dimensional quantity, but it is unfortunate that it should be used indiscriminately in both senses. Nor can the trouble be overcome by considering only one aspect of the matter and deliberately shutting our eyes to the other. The truth is that two different names are needed, as in the strictly parallel case of specific gravity and density. Until these have been agreed upon I think there is a case for defining specific heat as a ratio, and using the term *thermal capacity per unit mass* for the dimensional quantity. Specific heat would then correspond to specific gravity, and would be the constant actually measured in "specific heat" determinations. In any case, I think that all definitions of the following type should be absolutely rejected :—

"The specific heat of a substance is the amount of heat required to raise the temperature of 1 gm. of it through 1° c."

This associates the entirely nebulous term "amount of heat" with the exact terms 1 gm. and 1° c. One might just as well define the density of a substance as the amount of matter contained in 1 c.c. of the substance.

Incidentally, I feel that there is a good deal of confused thinking even on the subject of specific gravity and density. Some teachers have suggested that the term specific gravity should be abolished, as unnecessary. The fact is that nearly all the common methods of experiment (e.g. specific-gravity bottle, hydrostatic balance, Hare's apparatus) determine specific gravities and not densities. Some teachers even go so far as to call a specific-gravity bottle a *density bottle*, though it is obvious that only specific gravities can be found by this method. Anyone who doubts this should provide himself with a balance, a specific-gravity bottle and a set of English weights and try to find the density of a liquid in pounds per cubic foot (without assuming someone else's result for the density of water in these units). The accurate determination of the density of a liquid is very much more difficult than a measurement of its specific gravity.

To return to the question of specific heat, I woud suggest the following as a consistent and logical approach to calorimetry :—

(a) Choose an arbitrary unit of heat, and any convenient scale of temperature.

(b) Define the *thermal capacity* of a body as the quantity of heat required to raise the temperature of the body one degree. Thus, thermal capacity is expressed in heat units per degree.

(c) Denote the *thermal capacity per unit mass* of any substance by c, and the thermal capacity per unit mass of some standard substance by c_0. Then c and c_0 are measured in heat units per unit mass per degree.

(d) Define the *specific heat* s of a substance as

thermal capacity per unit mass of substance/thermal capacity of unit mass of standard.

Thus $s=c/c_0$, and is a pure number.

On this basis, the thermal capacity of a body of mass m and specific heat s is msc_0 *heat units per degree*, and the "standard"-equivalent of a calorimeter of mass m' and specific

heat s' is $m's'$ *units of mass*. In a specific-heat determination by mixing a hot solid with the cold standard substance we have an equation of the type

$$msc_0(t_2-t_1)=m_0c_0(t_1-t_0)+m'sc_0(t_1-t_0)\ ;$$

whence

$$s=(m_0+m's')(t_1-t_0)/m(t_2-t_1).$$

Thus we obtain s as a pure number without requiring to know c_0, and the masses and temperatures may be expressed in any units whatever without affecting the result. The method of cooling and electrical methods can be treated in the same way.

It may be, and generally is, convenient to choose our units so that $c_0=1$, but this is not essential any more than we are obliged to use units in which the density of water is unity. It may be noted that if we use absolute c.g.s. units, the value of c_0 for water is not unity, but $4{\cdot}2\times10^7$ ergs per gm. per °c.

I agree with Mr. Hansel, in his paper on Mass, that students of average intelligence are quite capable of understanding absolute units, and of learning the correct use of the terms *mass* and *weight*, and I support his view that the second law of motion should be applied in the form $F=ma$. The equation $F/W=a/g$ is neither necessary nor desirable, and momentum, kinetic energy, and moment of inertia should be expressed as mv, $\frac{1}{2}mv^2$, mk^2, and not as wv/g, $wv^2/2g$, wk^2/g. It seems to me to be fundamentally wrong to introduce some local value of g into formulae and expressions which have nothing whatever to do with gravitation, and which are presumed to be of general applicability, not only on the earth, but on any other celestial body, or, indeed, at any point in space. In a consistent system of engineering units a special unit of mass is undoubtedly required, and although the name *slug* seems to me to be peculiarly ugly, I agree that it is much better to use it than to confuse our students by pretending that the engineer's unit of mass is the pound, and then trying to put matters right by introducing chaotic expressions and equations. But is it really necessary for students of physics to be taught engineering units as a separate system? Is it not sufficient to give a general instruction to beginners to convert everything into absolute units before substituting in a dynamical formula or expression? Students soon acquire the habit of converting miles per hour into feet per second before substituting in a kinematical equation, and it is equally easy for them to learn to convert pounds weight into poundals before substituting in $F=ma$.

I think that Hansel's statement that a beam balance measures *mass* and a spring balance *weight* is too sweeping and needs further discussion. The fundamental formula for a beam balance in equilibrium is $m_1g_1d_1=m_2g_2d_2$, which expresses the fact that the weights on the two sides have equal moments about the support. The balance is constructed so that $d_1=d_2$, and experiments with pendulums show that g is the same for all kinds of matter at a given place, so that ultimately $m_1=m_2$, and the balance does in this way determine equality of mass. In a spring balance the weight mg is balanced against the tension in the spring, but the graduations are obtained by hanging standard "weights" on the spring, so that we are again comparing m_1g_1 with m_2g_2, but this time with the spring as intermediary. Thus, if $g_1=g_2$, we again have $m_1=m_2$, and the graduations represent masses, not weights. Thus it seems to me that there is no real distinction between "weighing" by beam balance and by spring balance. The spring balance is, of course, only calibrated for one value of g, and, therefore, gives different readings in different places; but I see no justification for using "weight" to express a spring-balance measurement and "mass" for a beam-balance measurement. I think that, in general, mass should be used in both cases.

I must also criticize Hansel's example on the hydrostatic balance on page 172. He writes: "Mass of solid in air=100 gm." "Mass of solid in water=80 gm." Surely this is completely false, or at least entirely misleading? How can the *mass* of a solid be any less in water than in air? It is completely contrary to Hansel's own statement earlier in the paper that teachers should stress the constancy of the mass and the variability of the weight of a given body. Would Hansel say that the mass of a body floating in a liquid was zero? Surely the hydrostatic balance is measuring the tension in the supporting thread, not the mass of the suspended body? I suggest that a consistent statement of Hansel's problem would be:—

Tension in supporting thread when body is in air = 100 gm. wt.
" " " " " water = 80 gm. wt.

I think, however, that we might be permitted to express the same thing more shortly, if slightly less accurately, by saying, "Weight of body in air=100 gm."; "Weight of body in water=80 gm."

Finally, I should like to express my warm thanks to Mr. Hansel for the valuable work he has done in collecting together the large quantity of material contained in these papers, and in setting it out in a form which cannot fail to be of great assistance to all teachers of elementary physics.

Dr. G. T. P. TARRANT. For the past three years I have been teaching my boys at their earliest stage to insert in their formula not only the numbers of the physical quantities concerned but also the units as well. Thus, Density is $\frac{24 \text{ gm.}}{3 \text{ c.c.}} = 8$ gm. per c.c.; $I = \frac{\pi r^4 l}{4} = \frac{\pi\, 2{\cdot}4 \text{ gm./cm}^3 \,(1{\cdot}0 \text{ cm.})^4 \times 10 \text{ cm.}}{4} = \pi \times 6 \text{ gm.cm}^2$. The boys then solve the unit as well as the numerical part. The advantage of this system is that it draws attention to the units, shows them immediately if they are making any mistake by confusing, say, inches and cm., makes them clearer on the difference between density and specific gravity and enables them to present their result with the correct unit attached.

It has also just struck me, though I have not tested it, that the difficulty of forces in lb. wt. or in poundals would largely disappear if this method were adopted and if, instead of lb. wt., we simply said lb.-g. and cancelled out the lb. with lb. but left the g standing.

The only feature which introduces any difficulty is the names that are given to composite units—it is generally better for the boys to write lb.cm.sec^{-2} than poundals.

Let us now turn from this to the question of the dimensions to be assigned to electrical and thermal units. We are interested in dimensions because they are of use to us. With their help we can check equations, solve problems and convert values in one set of units to values in another set. In mechanics we start with length and time and find the units of acceleration. We then meet the equation $P=mf$, one equation containing two new quantities P and m. We must fix one arbitrarily—mass. It would be silly to say that we won't worry about the dimensions of this new quantity, mass. If we did we would lose a portion of the richness coming from the theory of dimensions. We could solve problems containing only two unknowns instead of the normal three. We could convert quantities from one set of units to another only if there was a change in the units of length and time, and could not deal with changes from gm. to pounds.

Now in electricity we encounter four equations connecting six unknowns, m, μ, e, ϵ (this is the dielectric constant), i and A. These equations are

$$\text{force} = \frac{m_1 m_2}{\mu d^2}, \quad \text{force} = \frac{e_1 e_2}{\epsilon d^2}, \quad \text{force} = \frac{mids \sin\theta}{Ad^2}, \quad \text{and} \quad i = \frac{de}{dt}.$$

It is usual for us to forget A because in the electrostatic and the electromagnetic systems A is unity, but A may not be unity in all systems.

The equation $c^2 = A^2/\mu\epsilon$ is not a fresh relation—as is often assumed. It is a derived statement coming from the above four equations, and should therefore be omitted.

Now, whenever we have four equations connecting six new quantities we must define new arbitrary units for two of the quantities. We always do this by defining these new units by reference to the values of a particular substance, i.e. vacuum. The unit of length is defined by reference to the distance between two scratch lines at the N.P.L. The unit of weight is defined by reference to the quality—mass of a particular substance, water—that is easily available. For the electrical quantities we choose for the particular arbitrary unit a more universal medium—a vacuum. In the e.s. and the e.m. systems $A=1$ and either ϵ_0 or μ_0 is made unity also. Two out of the three must be fixed arbitrarily with arbitrary standards, so that *two* new units should be introduced into our theory of dimensions if it is to retain all its potential richness. We can, if we like, omit A and use only four. But if we do, we can solve problems involving four and not five unknowns. We will not be able to change our values to other systems in which A is other than unity. We have lost something by being lazy.

So it is with thermal units. We have one equation,

$$\text{heat} = SM\theta,$$

connecting three new quantities—heat, S and θ. We must therefore fix arbitrary units to two of these—generally heat and θ. When we do this we should introduce these two quantities in our dimensional equations. If we introduce only θ, our dimensional theory cannot change from one set of units of heat to another. We will be able to change our derived units from ° C. to ° F., but we will not be able to change from the calorie system to, say, the work scale.

This meeting has now convinced me of the incorrectness of my previous argument that since heat = (mass of water)(temperature rise in ° C.), we are justified in neglecting heat in our dimensional equations. The calorie is a new arbitrary unit, defined by reference to ° C. and to the weight of water. It differs fundamentally in no way from the unit of mass—a new arbitrary unit defined by reference to a volume of water. If we omit it we are deliberately throwing away something which can be of use to us.

Mr. G. H. West. I am quite sure in my own mind that Mr. Hansel's ideas are right and that his method of dealing with elementary dynamics by starting with *mass* as a quantity of matter is the proper way to start with boys. If you begin with W/g they do not seem to grasp the fundamental principles and they are confused from the start. I would proceed in this way :—

Mass of body is the quantity of matter in the body.

Unit mass is the quantity of matter in a piece of metal selected for the purpose (the standard pound).

Forces are measured by the accelerations they produce in the unit mass.

Unit force is one that produces unit acceleration in unit mass.

Equal masses are those which balance each other on a beam balance.

Density is mass of unit volume.

I would abolish the use of the term specific gravity in school, and leave it for technical purposes only, for example, specific gravity of milk and beer.

Author's reply :

Paper I—Ohm's Law. I cannot think of any complete circuit to which Ohm's law can be applied accurately in the form *current* $= E.M.F./$*resistance*, the resistance being accurately constant.

Ohm's law, that for a metallic conductor under fixed physical conditions the ratio of P.D. to current is constant, applies accurately to metallic conductors but seldom applies with the same degree of accuracy in other cases. It is the exception rather than the rule. Of course, the definition of resistance as the ratio of P.D. to current applies in all circumstances, whether Ohm's law is applicable or not, but, in general, this ratio is not constant.

In an experiment to illustrate Ohm's law I do not think that it is necessary to calibrate the ammeter and voltmeter by a method independent of Ohm's law in order to illustrate or to " prove " it (if it can be proved or verified by a single series of observations). A law represents universal experience, and consistency of deductions from it is the only way of establishing a law. I do not consider that the experiment described would be improved by calibrating the ammeter and voltmeter by any particular method or by substituting a tangent galvanometer for the ammeter and a potentiometer or capillary electrometer, etc., for the voltmeter. I think it is legitimate to assume (1) that the instruments used are reasonably accurate, (2) that a voltmeter measures P.D. and an ammeter measures current quite independently of any method used to graduate these instruments. Ammeters and voltmeters are graduated by methods which secure consistency of readings of different instruments calibrated by different methods, and the methods of calibration used to-day use the experience of a past generation in ensuring that the instrument shall read what it is intended to read. The same instruments may be used to show that Ohm's law applies to a metallic wire and that it does *not* apply to a radio valve. If the metallic wire did not conform to Ohm's law, the measurements would reveal the nature and extent of the departure from it. The experiment described in my paper *does* illustrate that for the particular wire under the conditions of the experiment the current is proportional to the P.D. applied to the wire, assuming that the voltmeter reads volts and the ammeter reads amperes.

I do not favour fantastic experiments specially designed to avoid instrument calibrations

depending on Ohm's law, e.g. a separately-excited shunt dynamo with constant field running at variable speed, so that the P.D. at the brushes may be varied and measured with an electrostatic voltmeter and the load current with a Kelvin balance.

Paper II—Specific Heat. I do not agree with Prof. Stead's preference of the ratio definition of specific heat and his rejection of heat per unit mass per degree.

In the method of mixtures :—

Heat lost by hot substance = Heat gained by cold substance
= so many heat units (H) after substituting for s_0.

Hence, $s = H/m(t_2 - t_1)$, i.e. heat units per unit mass per degree.

Similarly in the method of cooling.

The procedure I advocate is to discard the term *thermal capacity* as unnecessary, and to use the term *specific heat* for what Prof. Stead calls the thermal capacity per unit mass. Then :—

(1) Define unit quantity of heat. There are many ways of doing this (see Article *Calorimetry*, Encyc. Brit., 10th or later editions, by H. L. Callendar).

(2) Define specific heat (s) as the quantity of heat per unit mass of the substance per degree. With consistent units, the specific heat is the same in all units, e.g. in calories per gram per °C. as in B.Th.U. per pound per °F.

If the temperature of mass m of a substance of specific heat s changes from $t_1°$ to $t_2°$, the heat change is $ms(t_2 - t_1)$heat units.

(3) Define latent heat of fusion or vaporization (L), in a similar way, as heat required to change the state of unit mass of the substance at the fixed temperature of its M.P. or B.P. Fahrenheit latent heats are 9/5 times Centigrade latent heats.

Heat for a change of state $= mL$, at a fixed temperature.

(4) Change of entropy $(d\phi) = dH/T°\text{K.}$, $dH = ms \,.\, dt$, and $\Delta H = mL$.

I do not agree that the method of mixtures or the method of cooling is only capable of interpretation on the assumption that s/s_0 is measured—this is one way of looking at it, but not the only way. On the ratio definition of specific heat, any heat equation involving both specific and latent heat would contain terms of different dimensions, e.g. the equation for finding the temperature when ice is added to a warm liquid.

The heat engineer and the student of thermodynamics are not particularly interested in the number of times the thermal capacity of a substance is greater than that of water. Nor do I think that students of physics, chemistry and engineering should regard quantity of heat as a " nebulous quantity ", nor that the equality between two quantities of heat should be concealed in the equations for the solution of heat problems.

As Prof. Stead points out, electrical methods give thermal capacity per unit mass per degree (which I call specific heat) which can be expressed in ergs (or joules) per unit mass per degree if J is known, or in heat units per unit mass per degree (using a double determination) if J is not known; and this is possible whichever definition of specific heat is adopted.

In my opinion, the type of definition which Prof. Stead recomends for rejection is to be preferred to the ratio definition for the following reasons :—

(1) Specific heat defined as heat per unit mass per degree enters into heat equations in a natural way, dimensionally correct, and without obscuring the nature of the thermal process.

(2) It is not essential to define unit quantity of heat as the thermal capacity of unit mass per degree of some standard substance. We may use the mean calorie between the ice and steam points (thus eliminating difficulties of thermometry and variation of specific heat); we may use as heat unit the quantity of heat required to melt unit mass of ice or some definite fraction of it, or the quantity of heat produced by a current of one ampere flowing through a resistance of one ohm, etc. Specific heat *not* defined as a ratio may be defined in terms of any unit of heat without any intermediary such as thermal capacity per unit mass per degree.

(3) If specific heat is a pure number, what is the expression for the change of entropy or internal energy of a substance when its temperature changes from $t_1°$ to $t_2°$? The ratio definition leads to an equation which is dimensionally unsound.

I do agree with Prof. Stead when he says "It seems to me a matter of little importance whether the term specific heat is used to express a ratio or a dimensional quantity; but it is unfortunate that it should be used indiscriminately in both senses." I do not agree that the two different names are needed, "as in the strictly parallel case of specific gravity and density". The cases are not strictly parallel. For one thing, density varies in numerical value with the system of units used, whereas specific heat used synonymously with thermal capacity per unit mass is the same in all consistent systems of unit.

In this connection an article by Prof. Ferguson on "Some Units and Definitions" which appeared in the *S.S.R.*, March 1937, vol. xviii, no. 71, pp. 347–351, is relevant. Prof. Ferguson concludes his article as follows :—

"To sum up: it would seem, then, that there is a distinction between thermal capacitance, as defined above, and specific heat, defined as a ratio; that thermal capacity should be expressed in calories per gram per degree, or some equivalent unit, and that a name should be given to the unit of specific heat, if the ratio definition be retained—it is merely an accident of definition that the number which measures a specific heat is the same as the number which measures a thermal capacitance; or, alternatively, and preferably, that the term specific heat be retained for what we have here called thermal capacitance, and that the ratio-definition be abandoned".

I find myself almost, but not quite, in agreement with the whole of what Prof. Ferguson says, and it seems relevant to this discussion to compare our points of view.

The equations of physics differ from those of pure mathematics in that the terms of a physical equation (i.e. the quantities separated by +, −, and = signs) are dimensional—each term consisting of a dimensionless number and a unit having the same dimensions for each term. The equations of pure mathematics contain dimensionless numbers only.

Physical quantities such as length, time, mass, force, current, etc., are continuously variable magnitudes which may assume all values within the range of physical experience. The measurement of these magnitudes involves :—

(1) The selection of a suitable unit U of the same dimensions as the physical quantity to be measured.

(2) The determination of the number of times n that the physical quantity is greater than the unit.

n is a dimensionless number—it is the ratio of two quantities having the same dimensions. The measurement is then expressed in the form :—quantity $Q=nU$.

Experiment indicates that certain relations exist between physical quantities. For example :—

1. The quantity of heat H absorbed or rejected by a substance during a change of temperature is proportional to its mass m and to the change of temperature (t_2-t_1):

$$H \propto m(t_2-t_1) \quad \ldots\ldots (1)$$

or

$$H=km(t_2-t_1). \quad \ldots\ldots (1a)$$

2. The mass of a substance m is proportional to its volume v:

$$m \propto v \quad \ldots\ldots (2)$$

or

$$m=kv. \quad \ldots\ldots (2a)$$

3. The quantity of heat H produced by a current I flowing through a conductor of resistance R for t seconds:

$$H \propto I^2Rt \quad \ldots\ldots (3)$$

or

$$H=kI^2Rt. \quad \ldots\ldots (3a)$$

4. The pressure p in any specified unit is related to the depth x in any specified unit, and the density of a fluid d in any specified unit:

$$p=kxd. \quad \ldots\ldots (4a)$$

The constant k in equations (1 a) and (2 a) is a physical constant for any one substance at a definite temperature and pressure. k is not a pure number but has dimensions.

Its value jumps discontinuously in passing from one substance to another. The value of such constants is not determined by comparison with a unit of the same kind, but by measuring the physical quantities involved in its defining equation, e.g. mass and volume in equation (2 *a*). My point of view here differs, I think, from that of Prof. Ferguson. I see no reason for using a unit of density or of specific heat. If units of mass, volume, temperature and heat have been defined, no further units of density or specific heat are necessary. Physical quantities are measured by comparison with a standard unit. Physical constants are evaluated by means of physical equations, which are also defining equations.

The constants k in equations (3 *a*) and (4 *a*) are constants of conversion and are dimensionless. The value of such constants is adjusted to suit the units of measurement of the physical quantities in the relation. The introduction of conversion constants into a relation to adapt it to fit chaotic units is a chaotic procedure, useless, confusing and unscientific.

Specific gravity (the ratio of two densities), specific heat (if defined as the ratio or two quantities of heat), coefficient of friction (the ratio of two forces), expansion ratio (the ratio of two volumes), efficiency (the ratio of two quantities of energy), etc., are dimensionless numbers or numerical coefficients, and need not appear in the equations of physics except where it is convenient to use a single symbol instead of two symbols and a division sign, e.g. expansion ratio, or where the ratio is used as a numerical coefficient such as efficiency or coefficient of friction.

The discussion here given relates to the physics of material systems in which quantities of matter, distances and time intervals are comparable in magnitude with those of ordinary human experience and perception. It does not follow that the same principles and treatment apply to the physics of minute space and time intervals (e.g. sub-atomic physics) to the ether (e.g. radiation, cosmic rays, etc.), or to the phenomena of light, electricity and magnetism (e.g. dielectrics and magnetic media). The discussion of these phenomena in terms of the ordinary theory of dimensions taking length, time, mass and temperature as indefinables, has led to much divergence of point of view. It may be that the same indefinables are now insufficient or irrelevant.

Paper III—Mass. Prof. Crowther has put his finger right on the spot in his reference to the insularity of some of the teaching in dynamics (see also Paper I, *Mass*, Appendix 4). Dynamics, taught as an isolated subject, leads to a confusion of ideas with regard to mass and inertia, or it may lead to the avoidance of the mass concept or even its complete elimination. To the physicist, chemist and engineer, the law of conservation of mass is of fundamental importance. This is not the law of conservation of inertia.

The mass of a body is the quantity of matter in it (Newton). Mass may be measured in any reasonable way whatever—weighing, titration, impact, inertial acceleration, gravitational acceleration, etc. The results by different methods agree. The terms *mass* and *inertia* are not synonymous. Inertia is only one of the properties of matter by which the measurement of mass is made possible, and this method is by no means the most convenient or the most accurate. I prefer the views of Newton to those of Mach or Poincaré (see Q. 74). Mach identifies mass and inertia, Newton does not.

The term *moment of inertia* is not well conceived. Nor are its equivalents, *second moment* or *second mass moment*. Why moment? Is this the changed meaning of *importanza*, the word used by Leonardo da Vinci? Moment of inertia measures rotational inertia. I think it would not only be unwise to pander to the vagaries of the popular usage of words and substitute inertia for mass, but the procedure would be inaccurate, unscientific and unsatisfactory.

What does a beam balance *measure*? It will be profitable to forget about rapidly-changing gravitational fields in space or time, or balances with beams of length to be reckoned in parsecs and of such great mass that the masses refuse to remain in the scale pans. Primarily, a beam balance compares moments: $m_1g_1d_1=m_2g_2d_2$. If $d_1=d_2$, it *compares* weights. If, in addition, $g_1=g_2$, it *compares* masses. If the weighing is performed at the place where the standard weight is defined and with standard weights (which is never the case) it *measures* weight, but in no other case. If the weighing is performed with standard masses, as is always the case, it *measures* mass. The beam balance gives consistently the same results everywhere. How, then, can it *measure* weight, which is not

the same everywhere? If it does not measure weight, how can it measure the tension in a string or any other force except with standard weights, where no-one ever weighs? So far as the beginner in science is concerned, a pair of scales has always measured mass (quantity of stuff) and not weight, but he has always called it weight and not mass. Is it not urgent to correct this misuse of the terms mass and weight? This is no more difficult than in the parallel case of heat and temperature.

The distinction between mass and weight should be taught early and at the very beginning of the science course. No slovenliness of speech or written record should be tolerated: there must be rigorous insistence on the correct use of the terms mass and weight. The teacher of mechanics should be careful to continue the work already begun in the science course.

"Mass in water" has been criticized. I entirely agree that this statement is thoroughly bad—mass is invariable. But is not the statement "weight in water" worse still? A beam-balance weighing is consistently the same everywhere. How can it measure weight or gravitational force, which are not the same everywhere? Apparent mass and apparent weight are even worse. Is the apparent weight of a body resting on a table nothing? What is the apparent weight of a picture hanging by an oblique cord? Surely the body has its natural weight independently of any particular method of support. In an Archimedes' experiment we are dealing with an unaltered weight and an additional force (the upthrust).

For my part, I do not think that this is a suitable subject for a beginner. At a later stage, when weight has been developed as a resultant of parallel forces acting at a centre of gravity, when composition and resolution of forces have been considered, and when the dependence of pressure on depth and density have been considered, the principle of Archimedes can be discussed and stated in the form that a body immersed in a fluid experiences an upthrust equal to the weight of the fluid displaced, and this upthrust acts at the centre of buoyancy. These ideas, coupled with the proportionality of mass and weight, will enable a student to state his problems intelligently and accurately, using the word mass or weight appropriately. Even a beginner cannot fail to notice the inconsistency of habitually using the word *mass* for beam-balance weighings and then suddenly using the term *weight* instead. This is also to be found in most text-books.

Density (or its reciprocal, specific volume) is the quantity that enters dimensionally into the fundamental equations of physics and engineering, e.g. the relation for the velocity of a sound wave through the air. Specific gravity is relative density, and a specific-gravity bottle may equally well be called a relative-density bottle if the volume of the bottle is not marked on it. But if the volume of the bottle is specified, this is for the purpose of determining density, and it is a density bottle (although I do not use this term myself):

$$\begin{aligned}\text{Specific gravity} &= \text{density of substance/density of standard,}\\ &= (m/v)/(m_0/v_0),\\ &= m/m_0 \text{ if } v=v_0, \qquad \ldots\ldots (1)\\ &= v_0/v \text{ if } m=m_0. \qquad \ldots\ldots (2)\end{aligned}$$

(1) is useful when dealing with a body that sinks;

(2) is useful when dealing with a body that floats.

The specific-gravity bottle, hydrostatic balance and Hare's apparatus determine relative density. If one of the liquids is the standard substance, the relative density becomes the specific gravity, but not otherwise.

If a beginner *must* deal with Archimedes' principle, then it should be presented experimentally. The difference between weighings of the body in air (really in a vacuum) and in a fluid is equal to the *mass* of fluid displaced. This is what an experiment with a beam balance discloses. That a beam balance measures mass and not weight is a matter of fact and not a matter of opinion.

Mr. Lauwerys complains of ambiguity and obscurity in the following statements:—

1. It is advisable to teach a student the conception of mass and not to ignore it completely.

2. That whatever form of treatment is followed, it will not be necessary to change over to some different type of treatment at a later stage. It would be better to defer the teaching of dynamics if the student is unable to grapple with its fundamentals.

3. Every student should be able to work in C.G.S. and in British units. All the better if he can work in any units—in fact, if he is properly trained he will be able to use all rational units with equal facility. Whatever point of view anyone may take on this matter, I think it will be conceded that if a type of training can be devised which enables all systems to be used with equal facility, or which applies equally to each of the rational systems of units, it is highly desirable that such a type of training be adopted universally. Any form of training which fits one system of units and does not apply to another, except by the introduction of unnecessary, unwanted and irrational units, should be discouraged.

There are four rational systems of units (see *Mass*, Appendix 5).

Rigorously and ruthlessly discard chaotic units such as the gram weight, grams weight per square centimetre. These irrational units are unnecessary, and in the same category as dyne-mass or poundal-mass.

A pressure may be expressed as a head of water or mercury, but avoid grams weight per square centimetre at all cost.

Taking Mr. Lauwerys' numbered points in order :—

1. The confusion of mass and inertia has been considered—the water-carts hardly merit it.

2. When Mr. Lauwerys says " we do not have to change over to a new system " and " we extend ", I refer to this as " a change over to some different type of treatment ". This may seem " obscure " to Mr. Lauwerys, but Section 6.5 of the Report of the Mathematical Association (p. 26) begins : " Those who commence with the use of gravitational units should, if they continue their studies sufficiently far, change over to the habitual use of absolute units (except for working certain types of numerical examples). It is recommended that the change should be effected in two stages, a preliminary stage in which absolute units are introduced and used for the first time, while gravitational units still remain dominant, and a final stage in which absolute units become habitual and gravitational units are merely reserved for problems to which they are specially suited."

Prof. Alfred Lodge's remarks are amusing when quoted against the use of $P=ma$, for he has most successfully revealed the kind of tangle that ensues when weight W is introduced into a simple problem in which it is irrelevant. Experiment shows that, for a given body, mass and weight are proportional, the constant ratio or the coefficient of acceleration being found to be equal to the mass of the body measured in other ways. Hence $P=ma$. Where is the relevance of W? Having established that $P=ma$ for forces in any direction, obviously $W=mg$.

Did Prof. Lodge state " The equality $P/W=a/g=$const. for a given body is more abstruse and should be delayed a bit " ? If so, I suggest it is so abstruse as to be incorrect, and should be delayed indefinitely.

I am not at all clear why Prof. Filon's remarks have been quoted. Does Mr. Lauwerys propose to *define* mass as W/g; or does he contend that it is not necessary to distinguish between mass and weight ? I follow Newton in defining mass as quantity of matter, and in measuring mass with a pair of scales. The mass (quantity of stuff) is the only real knowledge of things which is unavoidable in ordinary experience. That a pair of scales measures mass or quantity of stuff has been verified over and over again in the common incidents of daily life.

3. The irrational, unwanted units are the gram weight and grams weight per square centimetre, etc. $P/W=a/g$ cannot be applied to absolute units, and necessitates the introduction of gram weight, etc. How does Mr. Lauwerys propose to define unit pole and unit current ? How is the gram weight to be used in the quantitative study of electricity and magnetism ? I only use the gram weight when compelled to do so (lever experiments, calibration of spring, with beginners). I should very much prefer megadyne or kilodyne weights.

4. The difficulties of Karl Hahn (and of his pupils) are understandable if they approach the concept of mass through Attwood's machine. The difficulties are doubly distilled if kilograms and kilograms weight are used as the units of mass and force.

5. Mr. Lauwerys cannot fail to have convinced everyone of the futility of discussing mass with beginners according to the methods he proposes. The remedy is to follow Newton.

6 and 7 have been answered by Prof. Crowther, Prof. Stead and Messrs. Brereton, Dyson and West.

The formula $P/W=a/g$ was introduced by Prof. Greenhill and used in teaching applied mathematics to military cadets and officers. He completely ignored the conception of mass, and dealt exclusively with gravitational units and weight. The questions set in the Army Entrance Examination for many years dealt only with gravitational units. Public schools with a military side specialized in this type of training, and not a few of the text-books of mechanics have emanated from masters in these schools. Once a student has become habituated to this type of training it becomes impossible to change his point of view. He has difficulty with the conception of mass, and this difficulty arises entirely out of his previous training. Actually it is not mass that he finds difficult, but how to introduce this quantity into the equation $P/W=a/g$. Give me a young untrained beginner and I have no difficulty in teaching the conception of mass, but in the case of the habitual user of $P/W=a/g$ I find it impossible to teach him anything—his invariable reaction is a blind application of the formula $P/W=a/g$.

The remarks by Prof. Crowther and Prof. Stead on the force equation and units are well worthy of study. I include in this remark Prof. Crowther's remarks on the slug—his thrust at me is justified. I agree that the placation of the "practical" people has been a bad blunder, maybe irretrievable, maybe not. For this reason I have argued "to do a great right, do a little wrong". The introduction of the slug would enable those who desire to add to the number of units to go their own way without queering the pitch for absolute units.

Finally, I am extremely grateful to all those who have contributed to this discussion.

PRESENTATION OF DUDDELL MEDAL

THE eighteenth Duddell Medal was presented to Dr. W. D. Coolidge at a dinner of the American Physical Society held at Washington on 1 May 1942. The presentation was made by H.M. Ambassador, VISCOUNT HALIFAX, who said :—

It is a great pleasure for me to be here to-night, and for such a purpose as is the occasion of our meeting.

The Physical Society has decided to award its 18th Duddell Medal to another eminent American investigator, Dr. William David Coolidge, who has for many years past directed the research work of the General Electric Company. You must not think that in requesting me to make this award on its behalf the Physical Society was under any illusion that it was choosing a representative qualified to speak in its name about Dr. Coolidge's work. My knowledge of modern science, I am afraid, is shamefully slight, but I can truthfully claim that my interest in it is perhaps greater than that of many who profit by the results of science in daily life in such ways as using a car, listening to the radio, or getting about the country by air. Moreover, when the Society's invitation reached me I naturally took pains to consult with such learned minds as I could find, in order to inform myself to the best of my ability about the nature and significance of the achievements for which Dr. Coolidge is being honoured to-night.

As a result of my enquiries I am able to state that this award has been made primarily in recognition of his invention of the type of X-ray tube which is now in general use both in physical investigations and for medical purposes throughout the world, and which is everywhere known by Dr. Coolidge's name.

Dr. Coolidge's discoveries place him high in the list of holders of the Duddell Medal—a list which includes such honoured names as those of Sir Charles Vernon Boys, Sir Ambrose Fleming, Professor Ernest O. Lawrence and the late Professor Albert A. Michelson.

In constant stream the world has received incalculable benefits from the work of scientists, and the frequency with which those benefits have been conferred upon us has steadily increased over the past two centuries. Since Newton discovered the law of gravity the work of scientists in every land has combined to transform almost out of recognition the world in which, for so many thousand years, man had been living his brief, uncertain and arduous life. The field of medicine has been particularly favoured, and I am told that few, if any, living scientists could match the benefit which Dr. Coolidge has conferred upon medical science in making the X-ray tube the most important accessory of medical diagnosis. Diagnosis has indeed been revolutionized, and the number of lives saved by X-ray inspection is beyond calculation. It can also be said with gratitude that Dr. Coolidge's invention has reduced the hazards to which the earlier workers in the field of X rays were exposed, and to which, alas, so many of them sacrificed both health and life.

Although Dr. Coolidge already holds many honours, both American and British, this has seemed to the Physical Society an opportune moment to honour his exceptional achievements once more, since not only does the Coolidge tube in wartime mean the difference between life or death to thousands of wounded, but his researches in ductile tungsten have led to a complete revolution in signalling and communications, as radio-telephony has become indispensable to any form of modern warfare, and in the field of communications is the chief medium through which the Allies are bound together.

The Officers and Council of the Physical Society rightly feel that the award of the Duddell Medal to Dr. Coolidge has to-day a significance deeper than the honouring of the achievements of a great investigator. For the men of science of our two democracies are united by a common belief in liberty of thought and expression, without which their work would lack both opportunity and meaning.

This war, I need hardly remind so distinguished a company, is different from all others, not merely in the vastness of its geographical extent nor simply in the fundamental and all-encompassing nature of the principles for which we fight. That is all true—but there is more than that. It is a war in which the whole apparatus of Western civilization is called into play to save or to destroy the essence of that civilization itself. Many, I know, find cause for little short of despair when they contemplate that fact. Science, they say, has made all this and science is now destroying it. Where and how, they ask, are we ever to break that vicious circle ?

Yet I do not by any means despair of seeing that question answered, for we now begin to see that the old opposition between natural science and the world of the spirit was unreal. The material world about us has expanded into an universe which scientists themselves admit only faith can comprehend. In this new and infinite world that has been opened for us, scientist and layman alike face the unknowable again. Realizing as we do the narrow limits of our human range, we shall, I think, come once more to the knowledge that the issue of our endeavours, each in his own walk of life, lies in wiser than human hands. Thus, I believe, it is not unreasonable to hope that the old conflict will resolve itself upon a higher plane and mankind go forward again with a new humility and a re-awakened faith.

Dr. Coolidge, on behalf of the Physical Society it is my pleasure to present to you the Duddell Medal for 1942, and the certificate of award and the honorarium that go with it.

In accepting the Medal, Dr. Coolidge said* :—

May I first thank Your Excellency for your great courtesy in coming here and giving me the pleasure of receiving this honour at your distinguished hands. This is generous compensation for the disappointment which I naturally felt because present conditions made it inadvisable for me to attend a meeting of the Physical Society in London to receive the award directly from their President.

I am deeply conscious and appreciative of the honour of being made a Duddell Medallist, and for several reasons. The medal takes high lustre from the great scientific achievements of Duddell, in whose honour the Physical Society established the memorial fund—a lustre enhanced by the eminence of that society itself and by the conspicuous merit of former recipients. It carries special significance and value for me in coming from England, the home of my ancestors, the source of many great achievements in science, and the stalwart guardian of what are to me the finer things of life.

To return for a moment to the work of Duddell. Had he done nothing more than develop the magnetic oscillograph, his contribution to the electrical science and art would have been outstanding. One cannot visit a laboratory today where electrical researches or electrical engineering developments are in progress without being greatly impressed by the importance of the rôle played by the oscillograph, which yields so much essential information obtainable in no other way. For frequencies within its range, the magnetic oscillograph, substantially as developed by Duddell, is still in general use. For higher frequencies, the cathode-ray oscillograph is used, but here also credit must be given to Duddell, for his magnetic oscillograph served to blaze the trail. The art of radio also is heavily in debt to Duddell for his discovery of the singing arc, his design of a high-frequency alternator, and of his thermo-ammeter, all of which contributed to the early development of wireless telegraphy.

To make clear the limitations of my part in the work on which the award was based, I must briefly outline the conditions under which the work was done.

Our laboratory, at the time the ductile-tungsten work was started, was only five years old. This laboratory sprang from the vision of Dr. E. W. Rice and his associates. He was then in charge of General Electric engineering and had seen clearly the extent to which the products of our factories had been derived from the results of scientific research, such as had been carried out notably by English physicists.

The laboratory was started in the hope that it might contribute new facts and principles on which new products could be based. It was one of the earliest industrial research

* Reproduced by courtesy of the American Institute of Physics from *J. Appl. Phys.* **13**, 478 1942).

laboratories in this country and was established primarily for fundamental research. It was itself an experiment, but an experiment carried out under extremely favourable conditions, as both its sponsors and its director, Dr. W. R. Whitney, were men of great courage, optimism, and faith in the power of scientific research methods. The strongest single psychological factor conducing to the success of the laboratory was the spirit of co-operation which Dr. Whitney was able to build up not only within the laboratory but also between the staff and the engineers of the Company.

That co-operation was so close and so generous and so effective that a very large share of the credit for the ductile-tungsten and x-ray work belongs to those who were associated with me in that work. Credit is also due to the courageous and liberal management of the Company in giving its continued support to such work as the ductile-tungsten effort, which over a long period of years was exceedingly expensive and of questionable promise.

While the factory process for making ductile tungsten is to-day fundamentally the same as that developed in the laboratory, it has undergone constant improvement and been given increased utility by our engineers for the various applications which have been found for the product.

Although this process did not constitute the first application of powder metallurgy, it seems to have been at the time the most important one which had been made, and for this reason it may perhaps be fairly credited with having been in no small measure responsible for the subsequent development of the art of powder metallurgy now widely applied to other metals, and to mixtures of metals and refractory compounds.

My participation in the work on x rays, which forms the other basis for the award, followed as a result of the keen interest which I had felt in x rays ever since the announcement of their discovery, also from the work which we did in adapting wrought tungsten to use as a target in the earlier x-ray tube in place of platinum, and from Dr. Irving Langmuir's studies on the thermionic emission of electrons from tungsten filaments in high vacuum. I had the advantage also of intimate first-hand knowledge of the limitations of the x-ray tube of that time and of the source of those limitations. Stable and controllable thermionic electron emission rendered unnecessary the gas content which alone had been responsible for the most troublesome limitations of the earlier tube. At the same time it provided the much desired flexibility of control and operation. I was also fortunate in having readily available not only tungsten wire for the cathode filament but also large masses of wrought tungsten which were at the time indispensable for the target. For when our first successful tubes were made, we did not know that it would ever be possible sufficiently to de-gas a composite target of copper and tungsten such as had been generally used in the earlier type of tube.

I cannot forego this opportunity to pay my tribute to Sir Owen Richardson who, more than any other, had laid the groundwork for our knowledge of thermionic emission.

From the beginning, it was clear that the high-voltage generating equipment which had been developed for use with the earlier tube was ill-suited to the hot-cathode type, and for this reason later developments of the hot-cathode tube and of the high-voltage source have, even up to the present time, gone hand in hand.

While some hot-cathode tubes of the first so-called universal type are still in use, this design has generally been supplanted by special tubes giving greatly improved performance for various specific uses.

For example, types have been developed for operation on unrectified alternating current. The characteristics of the tube have permitted various forms of equipment in which the tube and the entire high-voltage circuit are completely enclosed within grounded metal, thus eliminating all danger of electric shock and facilitating the attainment of adequate x-ray protection. For radiography, definition has been greatly improved in various ways, notably by the use of elongated focal spots and, more recently, by this combined with rapid rotation of the target.

For medical therapy and for industrial radiography, tubes for higher voltages than could be used with the first design have been developed, first by merely increasing dimensions. Then, because of troublesome field currents arising from too high potential gradients at the cathode, we were led to depart from the simple two-electrode structure and to use a multiplicity of tubular accelerating electrodes in addition to cathode and anode. This apparently makes it possible to provide tubes for voltages as high as can be produced.

A recent development in the generation of high voltage to supply such tubes is the resonance transformer. This has no iron within the primary and secondary coils, thus permitting the x-ray tube to be located here, where it is electrostatically shielded by the surrounding coils. The size and weight of such equipment have been greatly reduced by substituting for oil a compressed gas having high dielectric strength.

For x-ray diffraction work, the recent development of tubes with linear focus combined with metallic beryllium windows results on the one hand in great reduction of the required exposure time, and on the other in a considerable extension in the long-wave-length direction of the available spectrum.

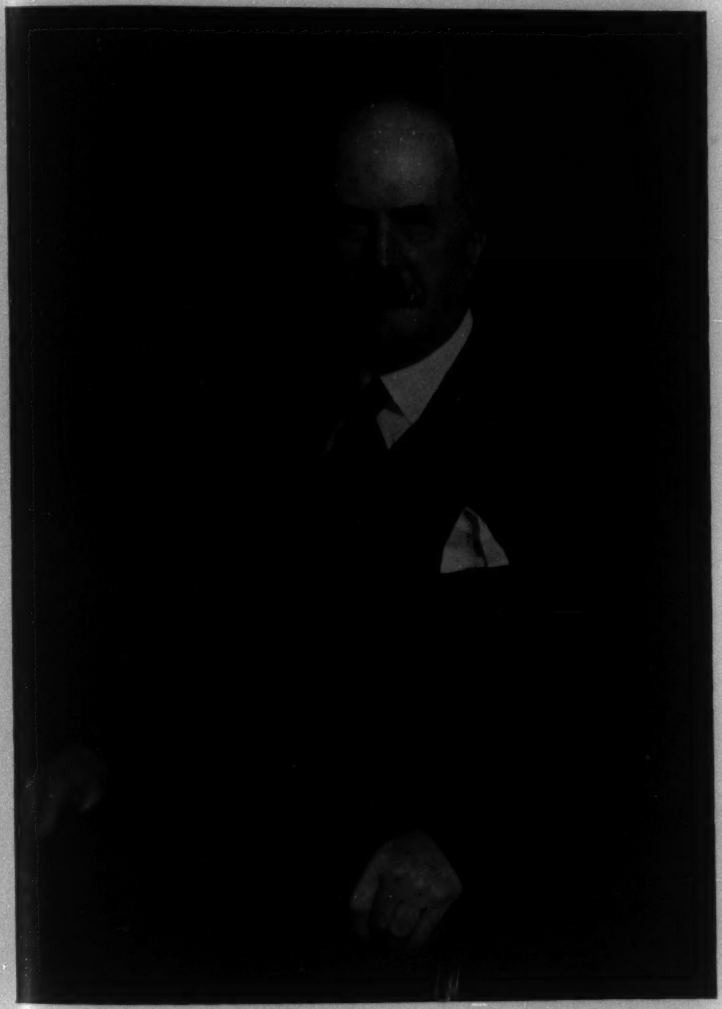
It is exceedingly pleasant to think of any useful purpose subserved by war. World War No. 1 did greatly accelerate both the development and the introduction to use of the hot-cathode tube. The present war has already led to the extensive use of the photo-roentgenographic method for the chest examination of recruits for the armed forces, and this, because of its greatly reduced cost, excellent results, and convenience, is certain to find widespread use after the emergency. The present war is also stimulating enormously the application of x rays to industrial uses, both radiographic and fluoroscopic. As an illustration of this, although the first portable million-volt radiographic x-ray outfit went into use only about a year ago, the fortieth one will soon have been completed. These million-volt outfits make possible the rapid examination of even as difficult subjects as steel castings and welds having a thickness of as much as eight inches, and thereby are contributing most helpfully to many industrial war activities. As another example, fluoroscopic outfits are being built and used for the rapid examination of small aluminium die castings, thus permitting immediate rejection of many whose defects could otherwise be seen only after a considerable amount of machine work had been performed on them.

To me, Your Excellency, the greatest value of this award lies in the fact that it is one of the many evidences of the cordial and friendly spirit which has for years existed between British and American scientists. This has been evidenced anew by the close and effective co-operation in the present war-work between the scientists of the British Commonwealth and those of this country. It has been greatly facilitated through your various war agencies in England, the National Research Council of Canada, and our Office of Scientific Research and Development. Your scientists are bringing us full reports of the vast amount of splendid work which you are doing in support of the war effort. We are making good use of it and are in turn keeping you informed concerning our work. This augurs well, both for the emergency and for the more leisurely pursuits to follow. This mutual aid in our common cause is strengthening the bonds between our nations, and we may hope that, when victory is won, those bonds may serve to hold us together in the task of rehabilitating the world on a firm basis of liberty, security and opportunity for all.

In every country of the world there are men of good will, who desire peace and friendly relations with other nations. Misrepresentative governments may gain temporary ascendancy, and by ruthless aggression and acts of cruelty bring upon themselves the hatred of their victims, but there remains, though temporarily submerged and silenced, the peace-loving mass of human beings who comprise the bulk of the nation, and who will respond in kind to an approach in the spirit of friendship and mutual helpfulness.

It is a far cry to-day, but we must certainly look hopefully to the time, and labour diligently for it, when such relations of mutual esteem and friendship as now exist not only between the scientists but also between the peoples of the British Commonwealth of Nations and the United States of America can spread to all peoples and to all countries.

In closing, Mr. President, I wish to express to the American Physical Society my gratitude for graciously providing this very pleasant setting for this occasion and thus lending endorsement to the honour conferred upon me by the Physical Society of London.



[*Photo*: *J. Russell & Sons.*

SIR WILLIAM BRAGG, 1862–1942.

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[*Photo*: *Special Press.*

SIR WILLIAM BRAGG, with some of the audience for his Christmas Lectures on "The Universe of Light", at the Royal Institution, 1931–2.

OBITUARY NOTICES

SIR WILLIAM BRAGG

WILLIAM HENRY BRAGG was born on 2 July 1862. His father, originally a master mariner, had in his later years become a farmer. Bragg was brought up on the farm and went to the village school. Later he was adopted by an uncle who lived at Market Harborough; then he went to King William College, Isle of Man. While there he won a scholarship at Trinity College, Cambridge, and this success determined his future career. He was Third Wrangler in the Mathematical Tripos in 1884. Having taken his degree, he had to look about for a position to earn as quickly as possible, and this circumstance may perhaps have prevented his entering for a fellowship. However this may be, his future career was, as he told me, determined by a curious accident. Being on his way to some academic function he had forgotten his gown. He hurried back to fetch it, and on his second journey he encountered J. J. Thomson in the street, who happened to mention that the post of Professor of Mathematics and Physics at Adelaide University was vacant owing to the resignation of the late Prof. Horace Lamb. The decision was imminent and Bragg had only time to apply for the post by telegram. He was chosen, though he knew little or nothing of experimental physics at this time. He took out books on the subject and read them on the voyage.

He remained at Adelaide for twenty-two years, and married in 1889 the daughter of Sir Charles Todd, the Postmaster-General and Astronomer Royal of South Australia.

Bragg's career as an original experimentalist began very late in life. Indeed it would be difficult, if not impossible, to mention anyone else who achieved great success in this direction without having made an earlier start than he did. He was, in fact, forty-two years of age when his first paper appeared in the *Philosophical Magazine*.

The way in which his original experimental work arose was this. He had been asked to give a public lecture in illustration of the newly discovered wonders of radium, and in order to illustrate it, and perhaps for his own instruction, he had procured a sample of the rather weak radium preparations which were then on the market. Arranging some lecture illustrations with this, he was led to ask himself certain questions which started his career as an original experimentalist.

His fundamental idea was this. The intensity of ionization produced by the alpha rays of radium falls off with distance owing to the rapid absorption of these rays in the air. Bragg, using a thin ionization chamber, determined the rate of this fall off, and using suitable methods confined the rays to a sensibly parallel beam. The remarkable fact then emerged that there were a number of discontinuities in the curve representing the ionization in a thin layer as a function of the distance of that layer from the source. These discontinuities were traced to the presence of the various component radioactive disintegration products, which Rutherford had detected as normally present in a radium

preparation. Each of these products had its characteristic range: that is to say, there was for each of them a characteristic distance to which the alpha radiation which it gave off could penetrate. There was this rapid falling off of ionization, the range being, in fact, a definite numerical constant, which could be specified for each of these component radioactive bodies. At the time when Bragg's early work began, such researches presented technical difficulties which it is not easy to envisage now that more powerful radioactive sources have become available. These early researches were made in collaboration with R. Kleeman, his demonstrator.

The value of Bragg's work was quickly recognized, and to some of the Cavendish Laboratory School it came as a surprise that a new star should have arisen in this way at the Antipodes.

Bragg left Australia in 1909 to take up the appointment of Professor of Physics at the University of Leeds, vacated by Stroud, of range-finder fame. The family returned home by the ill-fated ship *Waratah*. Her behaviour during this voyage, which was her first, gave cause for grave anxiety, and Bragg was often consulted by the commander during the voyage. The ship disappeared without a trace on her next voyage between Durban and Cape Town, and Bragg was able to give scientific evidence at the enquiry which was based on his observations of her stability, and was helpful in elucidating the probable causes of the ship's mysterious disappearance.

He only remained a short time at Leeds, being invited to take up the post of Professor at University College, London, in 1915. It was during this particular period in England that his most famous researches were made. He was quick to take advantage of the discovery by Laue of the diffraction of **x** rays by crystals. Laue's original point of view had been that the crystal acted as a three-dimensional grating, its spacing being such as to be suitable for the short wave-lengths of **x** rays. Bragg's earlier contributions were chiefly in the direction of applying this discovery to the examination of x-ray spectra. In this work he was associated with his son, and he was always careful to explain that the guiding idea of these researches, which assimilated each layer of atoms in the crystal to a reflecting surface, was due primarily to his son. These layers were uniformly spaced, and if the angle of reflection was varied, the reflected rays from the successive planes might or might not be in phase, according to circumstances. Owing to the large number of parallel reflecting surfaces which came into the question, the angle at which reinforcement was obtained was very critical and the resolving power correspondingly great. It was accordingly possible to determine the wave-length of x rays or, if this was regarded as given, to determine the spacing of the crystal planes with accuracy, and the subject suddenly jumped from a condition of vagueness and uncertainty to one of full precision with measurements making everything clear and definite within 1 % or less. He built the x-ray spectrometer, and one of its first successes in his hands was to prove the existence of x-ray spectra. He worked out the $h\nu$ relationship, and the critical absorption and its relation to atomic weight. In the meantime his son was working out crystal structures, and he himself turned more in this direction after the war. This field of discovery is too well known and descriptions too generally accessible to make it

desirable to devote much space in this notice to elaborating the details. Bragg and his son have given in their books admirable summaries of their work and the development which has been built upon it by their successors. It was found possible to map out precisely the position and absolute distances of the atoms of the various elements in all the ordinary inorganic and organic crystals. The general conclusions of crystallography which had been gained by examining the angles of the crystal faces and classifying the various degrees of symmetry were all confirmed and illuminated by precise allocation of the positions of atoms in the crystal lattice. It would perhaps be fair to say that the bare bones of geometrical crystallography were clothed in flesh. Bragg and his collaborators went far to exhaust this field of research, and the work stands for all time as a strong and firmly built structure complete in itself.

During the war of 1914–18, Bragg was appointed a member of the Admiralty Board of Invention and Research under Lord Fisher, and applied himself wholeheartedly to the problem of the acoustical location of submarines. He was in charge of the experimental station, first at Hawkscraig and then at Harwich. In spite of the energy and patriotic enthusiasm expended in this work, the ultimate practical outcome was not great, and it is no secret that quite different methods are now employed.

In 1923 the position of Director of the Royal Institution became vacant by the death of Sir James Dewar, and Bragg was offered and accepted the post. It was commonly believed in scientific circles that after many years' experience he was tired of elementary teaching and the routine duties of an ordinary professorship, and was glad to escape into a different and in some respects less strenuous atmosphere.

The position of Director of the Royal Institution carried with it the tenancy of the flat at the top of the building in Albemarle Street, which in former times had been occupied by Faraday, by Tyndall and by Dewar, and here he made his home. The atmosphere of the Royal Institution was congenial to him. It was, as he said, a friendly place, and he was by natural gifts well qualified to carry out the social duties attaching to it, in the way of receiving the members after the Friday Evening Lectures, occasionally entertaining foreign scientific guests of distinction at dinner and otherwise, and generally contributing to maintaining the social traditions of the place.

It fell to him to give one or more courses of afternoon lectures in the course of the year. He was particularly successful in giving the famous Christmas Lectures, which had been initiated by Faraday and have remained an important feature of the programme of the Institution ever since. The courses he gave in this way were: "The World of Sound", "Concerning the Nature of Things", "Old Trades and New Knowledge", "The Universe of Light". These were afterwards published in book form.

It will be noticed that in selecting the topics for these and other courses he went considerably outside the range of his own speciality. Indeed, he rather leaned towards the older physics, which lent itself more easily to experimental illustration, with which he took great pains. He did not aim at spectacular successes of the music-hall type, or, to express it otherwise, his illustrations were directed to making the subject clear rather than to gaining personal kudos for the

lecturer. He would carefully discuss beforehand with his entourage what degree of detail would be suitable to the audience and how far it would be safe to go beyond the easier and more superficial aspects of the subject. In particular he leaned to the use of models in making his topics clear. He was well acquainted with all the traditional methods in experimental illustration developed by Tyndall and others, and had evidently studied them in detail during the period of his professorship in Adelaide. He looked on everything from the point of view of the audience, and was careful never to be too far over the heads of the majority.

The care and trouble taken in collecting and arranging the material for these lectures was very great. Bragg was not content to rely on what could be found in books or in original printed sources. He also gathered information in factories or workshops. He estimated the time spent in preparing for one hour's lecture at not less than a month.

He took a great interest in the history of the Royal Institution. He gave lectures from time to time on a variety of topics in this connection. These included courses or single lectures on "Acoustical Problems treated by [the late] Lord Rayleigh", "From Faraday's Notebooks", "Two Old Friends of the Royal Institution (Spottiswoode and de la Rue)", "Benjamin Thompson, Count Rumford", "History of the Vacuum Flask", "Tyndall's Experiments on Magne-Crystallic Action", "Experiments from the Researches of Sir James Dewar". He was particularly interested in all Faraday relics and Faraday traditions, and gave much attention to the editing of Faraday's notebooks in connection with the centenary celebration in 1931, which was largely organized by him. Apart from the subjects already mentioned, his Royal Institution lectures dealt for the most part with topics connected directly or indirectly with x-ray crystal analysis.

In the course of Bragg's administration it was found necessary to make important structural changes in the Royal Institution. Some of those to whom the place was holy ground regretted the necessity for this, which seemed something of a desecration, but there is no doubt that Bragg acted with a due sense of responsibility and with the full backing of the officers of the Institution. In particular the rising seats of the theatre were originally carried on a structure of wood, and the managers of the Institution were advised that the dangers of fire raised possibilities which were terrible to contemplate, particularly in the case of an audience of young people as at Christmas time. They felt it impossible to carry the responsibility of this risk, and although the Local Authorities had no power to compel action, they strongly advised it. The theatre was reconstructed with iron supports for the seats. After this reconstruction it was in the main replaced as nearly as possible in its original form, the historic lecture table being retained and the shape of the room left unaltered. The old Physical Laboratories used by Tyndall and Rayleigh ceased to exist as such, and the place was remodelled to provide a new series of rooms, including improved lecture-preparation room, librarian's room and other conveniences. At the same time the Laboratories of the Institution were merged more completely with the Davy-Faraday Laboratory, and the machinery used by Dewar for the liquefaction of gases, which had now become obsolete, was removed, with the exception of a few small pieces of historical interest.

On the more serious scientific side arrangements were made by which a certain number of salaried workers of reasonable maturity could be available to work under Bragg in the Davy-Faraday Laboratory. The original design of Dr. Ludwig Mond, the founder of the Laboratory, had been that independent workers with means of their own would be able to come there to work on their own problems, but in practice the scheme hardly worked out in the way expected. There were not enough workers of the kind contemplated to make proper use of the facilities of the Laboratory, and it was very desirable that younger men who had not an independent income or position, but whose creative activities were still on the east side of their meridian, should be encouraged to work at the Laboratory, and this change was made possible by Bragg's reorganization. He collected the necessary funds from a variety of sources. Some large industrial concerns were generous contributors. Among the workers with him from University College were A. Müller and G. Shearer. Other well-known workers were W. T. Astbury, J. D. Bernal, C. F. Elam (Mrs. Tipper), R. E. Gibbs, K. Lonsdale, and several others of comparable distinction. They worked for the most part on the problems of crystal structures which Bragg and his son had initiated, developing these more particularly on the side of organic chemistry, and later with reference to the structure of animal tissues and the like.

The researches of Bragg and his school were made for the most part without specially powerful appliances. The high-power x-ray installations (5 kw. and 50 kw.) developed by Dr. A. Müller for the more exacting problems of crystal analysis were perhaps the only exceptions.

Bragg was President of this Society for the years 1920–22 and delivered an address on the structure of organic crystals, which he had recently investigated by x rays. From 1935 to 1940 he was President of the Royal Society. He felt, as he told me at the time, a sinking of the heart at having undertaken this latter office, apparently from an entirely uncalled-for feeling of inadequacy and doubt as to his ability to carry the burden at the somewhat advanced age of 73. In fact, however, his energies lasted well. He had a strong hold on the respect and affection of the Fellows, and this was by no means the last of the public duties he was called on to perform. Towards the end he had to husband his strength, and usually took a taxi even for the short journey from Albemarle Street to Burlington House.

He was created K.B.E. in 1920 and given the Order of Merit in 1931, and was awarded at various times the Rumford and Copley Medals of the Royal Society. He died at the Royal Institution on 12 March 1942. RAYLEIGH.

WILLIAM ALFRED BENTON

BORN 16 March 1871, eldest son of William Benjamin Benton of Handsworth, William Alfred Benton was educated at King Edward's School, Birmingham. He received his engineering training with Frank Stacey & Co., milling engineers, of Birmingham, and then practised as a consulting engineer in Birmingham. About this time he became interested in automatic weighing machines and took out a number of patents.

During the last war he erected and worked a by-products plant for the extraction of ammonia and important organic compounds from waste material.

After the war, in 1921, he joined W. & T. Avery, scale-makers, of Soho Foundry, as head of their Research Department.

He had a great knowledge of the theory and practice of weighing, and in recent years put together for W. & T. Avery a unique collection of photographs, models and documents relating to the history of weighing from the earliest times. A few years ago he gave a demonstration to the Society on this subject. Amongst many other devices he invented an instrument for the rapid determination of specific gravities.

Benton was interested in optics and had patented an epidiascope, as well as carrying out some experimental work on photographic lenses. He was a keen student of literature and history, and some years ago, under the pen-name of John Gentleshaw, published a long poem, *The Observing Angel*, dealing with the economic depression.

He had been a fellow of the Physical Society since 1924, and was also a member of the Chemical Society, the Royal Photographic Society, the Newcomen Society, and the Birmingham Metallurgical Society, of which he was President in 1933–34. This Society had also awarded him their Silver Medal.

He died suddenly at his house in Handsworth on 19 June 1942.

FRANCIS JAMES SELBY

F. J. Selby, an early Fellow and Member of Council of the Optical Society, and also for long a Fellow of the Physical Society, died on 5 March. By training a mathematician, he was first a student of University College, London, and later of Trinity College, Cambridge. Some of his recollections of that time will be of interest, at least to a number of the older members of the Society. He was very decided in his view that Karl Pearson was the best mathematical lecturer he had ever met. At Cambridge he graduated as sixth wrangler in 1891, a year after Miss Fawcett was placed above the Senior Wrangler. It happened that Selby knew somewhat intimately the work of Miss Fawcett and of some men in the same year at Cambridge, for they had worked together at University College. However able Miss Fawcett may have been, she had been easily surpassed by one of the men who went from University College to Cambridge. At Cambridge, while the one worked steadily, the other occupied himself in other ways, and took a high place in the tripos on the work he had done before going up. Selby was sure that had he given a moderate amount of time to mathematics at Cambridge he would have been far ahead of everyone else in his year.

After leaving Cambridge, Selby became mathematical master first at Bristol Grammar School and later at Repton. In 1903, two years after its inauguration, he joined the staff of the National Physical Laboratory to take charge of the prediction of tides in the Indian Ocean, then about to be transferred to the Laboratory, and to start an Optical Division intended to extend the range of optical work beyond that carried out in the Observatory Department at Kew. To these duties Selby added that of Secretary to the Director, an office which

absorbed his time increasingly. The excellent relations he established with the whole of the Laboratory staff, combined with his good judgement and administrative gifts, rapidly led him to a key position in the Laboratory organization. This was only formally recognized in 1918, when he was appointed Secretary of the Laboratory, but in fact the general business of the Laboratory had been largely in his hands for several years: this greatly assisted the Laboratory in giving such notable service to the country throughout the war period 1914–19, when the Director, Sir Richard Glazebrook, was called on to spend much of his time elsewhere.

In 1909 the Advisory Committee for Aeronautics was formed, with Glazebrook as Chairman and Selby as Secretary. These additional duties led Selby to relinquish charge of the Optics Division. He continued as Secretary of the Committee until, in 1919, it was replaced by the Aeronautical Research Committee. He was awarded the C.B.E. in recognition of his services. He retired from the Laboratory in 1932 on reaching the age of sixty-five.

Selby had set himself the task of writing the early history of the National Physical Laboratory on his retirement. His knowledge of both men and events marked him out as the ideal writer of such an account. Unfortunately, before the work was completed he had a stroke which affected his memory, and the work was unavoidably brought to an end.

Throughout his service at the Laboratory Selby maintained his interest in mathematics, giving particular attention to relativity and the quantum theory. He took charge of the scheme instituted at the Laboratory for the continued education of junior members of the staff and for securing the best industrial experience for mechanics trained in the Laboratory workshops. In all his work he was inspired by a high sense of the service that the Laboratory should render to the Nation, and set a great example of unselfish devotion, often in circumstances of peculiar difficulty. T. S.

BERNARD MYNOTT NEVILLE

NEVILLE was born in 1886, and may be said to have spent his whole life in the atmosphere of the school. From 1899 to 1905 he was a pupil at William Ellis School, and he joined its staff in the latter year. Meanwhile, he continued his own studies, and after winning a scholarship at London University, he took his degree in 1909. He joined the Physical Society in 1910, and often attended its meetings. His own interest, however, was in the more academic bodies; he was treasurer of the S.M.A., and then advertisement manager of the *School Science Review*, and served long on its Advisory Committee. For 22 years he had been an examiner in Physics in the University of London.

That he lived such a full life, and was always cheerful, despite the handicap of lameness from which he had suffered since the age of three, is no small tribute to his spirit and character.

He died on 11 April 1942, leaving a widow, whom he had married as recently as December 1940.

REVIEWS OF BOOKS

Mass Spectra and Isotopes, by F. W. ASTON. Second Edition. Pp. xii + 276. (London: Edward Arnold and Co., 1942.) 22*s.* 6*d.*

The appearance of another edition of a book which occupies as historic a place as does this one in our academic and scientific literature is a most welcome event. The five chapters comprising Part I ("Historical") of this edition and the first four chapters of Part II ("Production and analysis of mass spectra"), while having been brought up to date, are not radically different from the first edition, but a tenth chapter, on "Modern high-power mass spectrographs", has been added, and includes those developments in experimental technique which have occurred during the important decade which has just elapsed. The title of Part III, "The elements and their isotopes", remains unchanged, but its form is completely altered. Instead of following the groups of the periodic system, it is now arranged according to atomic number, and a wealth of information, based on results published up to 1941, is contained therein. Part IV ("Theoretical and general") is much the same in form, but has been extensively revised. This second edition is a most worthy and useful successor to the first. W. B. M.

Time and the Universe: a new Basis for Cosmology, by [the late] F. L. ARNOT. Pp. 76, with plate. (Sydney: Australasian Publishing Company, Ltd., 1941.)

In 1937 the late Dr. F. L. Arnot, then a lecturer in Natural Philosophy in the University of St. Andrews, discussed with me a theory of time and cosmology of which a short account was published in *Nature* on 25 June 1938. This he subsequently expanded into a monograph which was ready for publication before he left Scotland in 1939 for his home university in Sydney. Here he rewrote and rearranged large sections in the endeavour to simplify and improve the presentation of his thesis. After his death in October 1940, publication was made possible through the generosity of the Scientific Publications Committee of the Commonwealth of Australia, and Prof. O. U. Vonwiller has attempted to carry out as editor what he believed to be Dr. Arnot's wishes as regards alterations.

According to Prof. P. W. Bridgman, the essential point to consider in defining any physical magnitude is the *process* of measurement—the rules of operation by which we obtain the measuring numbers associated with the physical phenomenon. When we apply this method to time, perhaps the most fundamental of all human conceptions, it is reasonable to inquire into the possibility of more than one time scale. For instance, there may be one scale for atomic phenomena, and another for ordinary mechanical phenomena. Such an assumption was made by de Sitter and appears in the cosmological theories of both Milne and Dirac. The atomic time-scale may be called the t-scale, while the scale obtained by using an ordinary macroscopic time keeper may be called the tau (τ) scale. This τ-time is measured by our ordinary clocks regulated by a pendulum or by a rotating planet. We then choose another time, called t-time, which is connected with τ-time by a logarithmic relation. It is then shown (not assumed) from the resulting theory that an atomic clock keeps this particular t-time, and that the age of the universe is finite. In *The Nature of the Physical World*, Eddington has said: "The difficulty of an infinite past is appalling. It is inconceivable that we are the heirs of an infinite time of preparation; it is not less inconceivable that there was a moment with no moment preceding it". To this Arnot replies: "Since neither time scale possesses any intrinsic claim to preference, we have two equally valid pictures of the universe. In one picture the universe was created at a definite epoch; in the other the universe has existed for an infinite time. Both views are equally correct."

The title of Chapter I is "The number of particles, mass and radius of the universe", that of Chapter II, "The bridge between cosmological theory and quantum theory". In some respects the next chapter, on the red-shift of nebular spectral lines, is the most important

in the book. The interpretation of this as a Doppler effect due to recession of the nebulae is a speculation which has unsatisfactory features. The present theory explains the red-shift as due to a decrease in the velocity of light in t-time, and the formula obtained for the degree of red-shift as a function of the distance of the nebula is in excellent agreement with the experimental observations. Moreover, a more decisive test of the theoretical formula will be possible when results have been obtained with the new 200-inch reflector.

Chapter IV is mainly mathematical, being concerned with relativistic t-mechanics, and the concluding chapter reviews certain selected subjects, such as the number of particles in the universe, the gravitational constant, and finally the relation between the two time-scales.

In the history of science, instances abound of the early statement of a new principle or theory being received with scant attention or even with active opposition. Time will show whether this new presentation of its own nature is of value in removing outstanding difficulties and in suggesting new investigations. H. S. ALLEN.

Spectroscopy and Combustion Theory, by A. G. GAYDON. With a foreword by A. C. EGERTON, F.R.S. Pp. x+191. (London: Chapman and Hall, Ltd., 1942.) 17*s.* 6*d.* net.

As a result of the systematization of molecular spectra on the basis of the quantum theory during the first decade or more after the last war, many of the band systems observed in the spectra of various flames were definitely attributed to emitting radicals, such as C_2, CN, CH, NH and OH, whose existence had hitherto been almost, if not entirely, unknown. At the same time the actual states of electronic and vibrational excitation of such emitters in flames and other sources of radiation were completely determined and described. Subsequent investigations led to an understanding of the presence and the rôles of these intermediate products in combustion processes and have yielded much new and valuable information on flame reactions.

The results of these recent applications of theoretical and practical spectroscopy to combustion problems are presented and discussed in this excellent monograph by the author of a short report on the same subject in the recent volume (vol. **8**, 1941) of our Society's *Reports on Progress in Physics*. The book is the first of its kind and can be unreservedly recommended to all who are engaged upon or interested in such problems.

An elementary introduction to the theory of molecular spectra is given in the opening chapter, and the conditions under which band systems appear in emission are discussed in the second chapter to provide an adequate theoretical background for the investigations, both qualitative and quantitative, described in later chapters. In the next six chapters the spectra of the hydrogen flame, hydrocarbon flames, cool flames, atomic flames, the carbon monoxide flame, explosions of hydrocarbons as in the internal combustion engine, and inorganic flames are described in some detail. Ch. IX deals with absorption spectra as a means of studying the progress of chemical reactions, as in slow combustion of hydrocarbons. Accounts of flame spectra in the infra-red region (Ch. X) and the calculation of life-times of activated molecules (Ch. XI) form a basis for discussions of the author's own work on the after-burning of carbon monoxide (Ch. XII), and of measured and calculated flame temperatures (Ch. XIII). Ch. XIV is devoted to the derivation of dissociation energies, and the final chapter to the application of spectroscopic results to the kinetics of the combustion process.

The usefulness of the monograph is enhanced by an Appendix of 20 pages giving numerical data and other information on molecular spectra and energy levels occurring in flames, by two plates of excellently reproduced flame spectra, and by an adequate index. W. J.

College Physics, by WILLIAM T. MCNIFF. Third Edition. Pp. 657. (New York: Fordham University Press, 1942.) $4.00.

The two volumes of the previous edition of this book are now combined into one very attractive volume with extensive revisions and much new material. Like several recent American books, it gives a little of almost everything in the vast field of physical

knowledge, from elementary mechanics of solids and fluids to modern nuclear physics. The text is broadly divided into seven parts : Mechanics (159 pages), Heat (68 pages), Sound (19 pages), Light (107 pages), Magnetism (17 pages), Electricity (198 pages), Modern Physics (38 pages). There follow a number of Appendices containing simple mathematical notes, formulae and tables, and information concerning the chemical elements and the electromagnetic spectrum. A large selection of numerical exercises, solved and unsolved, is included.

One of the author's main objects is to lay a foundation for more advanced study in each branch of the subject. Modern ideas are introduced wherever possible, not only in Part 7, which is entirely devoted to investigations of the last half-century, but in other parts of the text ; indeed, the subject matter of three whole chapters of Part 6 (Ch. 45 on conduction of gases, x rays, thermionic emission, rectifier tubes, the cathode-ray oscillograph, isotopes and the mass spectrograph ; Ch. 46 on radioactivity and cosmic rays ; Ch. 47 on radio telegraphy and telephony, television and the electron miscrocope) might well have been transferred to Part 7, which at present consists of one chapter (Ch. 49, on atomic structure, quantum theory and nuclear physics). These four chapters form perhaps one of the best features of the book. Two others which call for special mention are Chapters 43 and 48, which give elementary accounts of generators and motors and alternating currents.

In the Introduction, the author stresses and exemplifies the dependence of medical science on the applications of physical principles, and in two chapters (rightly described in the preface as foreign to the usual text-book of physics) he outlines some such applications, namely, physiological and therapeutical effects of changes in air density (Ch. 5), and electricity as a therapeutic agent (Ch. 44). These two, and several other chapters, render the book especially suitable for the medical student.

A few minor slips have been noticed : the ray-tracing in fig. 99 (D) on p. 277 is wrong; in problem 28 on p. 468 " kilowatt " should be " kilowatt-hour " ; on p. 510 " e/n " appears in one place for " e/m ". It seems unfortunate that while the protons-and-neutrons structure of atomic nuclei is represented in the very useful and informative chart on pp. 607–8, the earlier conception, protons-and-electrons, is mentioned alone on p. 580 and as an alternative on p. 610. W. J.

Wave Guides, by H. R. L. LAMONT. Pp. vii + 102. (London : Methuen and Co., Ltd., 1942). 4*s*.

The study of wave guides has recently become one of considerable importance because of their many practical applications with very short electromagnetic waves. During the last few years many authors have written accounts of work, both theoretical and experimental, on this subject, unfortunately not all using the same nomenclature. The present volume deals with the essential aspects of the theory, and includes most of the important work on the subject to date, with a uniform set of units and nomenclature.

The monograph is about 100 pages in length, with six chapters. The first chapter, after a brief historical survey, deals with the fundamental electromagnetic theory, and then with the rectangular guide, for which type the analysis is most simple. In the next chapter the author discusses general transmission theory, circular guides, dielectric guides, guides of elliptic and other special types of cross-section : the horn is also considered. Chapter III is devoted to the study of the attenuation and stability of waves in the guides; the losses due to the finite conductivity of the walls and to the conductivity of the dielectric are calculated. In chapter IV the author deals with the question of the wave impedance of a guide; then, on the basis of the simple laws of geometrical optics, a physical picture of the propagation of waves along a guide is given. The last two sections are devoted to the use of wave guides as resonators and as radiators. Resonators of various geometrical forms are compared and the relevant energy relations developed. The applicability of guides and horns to the production of beam radiation is indicated in the last chapter.

As the author mentions in his preface, the experimental side has received scant mention; an excellent classified bibliography is provided, however, so that reference to the experimental technique is facilitated. Dr. Lamont is to be congratulated on producing such a useful summary of the more important elements in wave-guide theory. J. A. S.

The Scientific Journal of the Royal College of Science. Vol. XII (Session 1941–42). (London: Joint Publication Board of the R.C.S., Imperial College Union, 1942.) 5*s.* 6*d.* cloth, 4*s.* 0*d.* paper.

It is the first duty of a reviewer to give his readers some notion of the content, scope and execution of a book. The content of such a work as this can be judged rightly only by listing the topics treated, and here, as briefly as may be, is a statement of the published lectures, given in the Session 1941–42 before the three Societies under whose aegis the Journal is published:—

CHEMISTRY: Electron Microscopy (A. L. G. Rees). Methane as an Engine Fuel (A. C. G. Egerton). Arrhenius, Ostwald and van't Hoff (A. Findlay). The Chemistry of Hashish (A. R. Todd). The Acetylene Alcohols (A. W. Johnson).

NATURAL HISTORY: Luminescence in Decapod Crustacea (R. Dennell). The Nature of Viruses (R. K. S. Wood).

MATHEMATICS AND PHYSICS: Graphical Methods in Teaching and Research (A. Ferguson). Electron Diffraction and Surface Structure (B. W. Soole). Some Physical Aspects of the Chemical Bond (A. Charlesby). The Differential Analyser (R. E. Beard). Diffraction Methods and Molecular Structure Determinations (A. Charlesby).

An admirably varied programme indeed, with fare to suit very different tastes. As regards scope and execution, the lectures are designed, and rightly designed, to appeal to the student trained in the subject of any particular lecture; they give him brief and clear surveys of the recent advances in that subject. But they do more than this. They give to the chemist an opportunity of learning something of advances that have been made in other branches of sciences, and conversely, workers in these other branches may learn something of the doings in the world of chemistry. This is a most useful function of the Journal, and one for which the present reviewer is happy to have this opportunity of placing on record his gratitude.

The volume is well produced, is, like its predecessors, thoroughly interesting; and there is nothing cheap about it, save its very modest price. A. F.

The Calculation and Design of Electrical Apparatus, by W. WILSON, D.Sc., M.I.E.E. Third Edition. Pp. xv + 240. (London: Chapman and Hall, Ltd., 1941.) 10*s.* 6*d.* net.

The second edition of this book, dated 1940, has already been reviewed in these pages. The speedy issue of a third edition evidences the serviceableness of the book, which has been amplified here and there, especially in the chapters on Heating and Heavy Conductors. D. O.

RECENT REPORTS AND CATALOGUES

Chance-Parsons Optical Glass. (Catalogue CP 400.) Pp. 19. CHANCE BROTHERS AND CO., LTD., Glass Works, Smethwick, near Birmingham.

Studies on Adhesives: Part I, *Ground-nut protein adhesives for plywood*, by D. NARAYANAMURTI, V. RANGANATHAN and P. K. BASU ROY CHAUDHURI; Part II, *Ground-nut protein—formaldehyde dispersions as plywood adhesives*, by D. NARAYANAMURTI and K. SINGH. (Indian Forest Leaflets, nos. 15 and 16.) Pp. 9 and 13. FOREST RESEARCH INSTITUTE, Dehra Dun, U.P., India.

Protection of Radium during Air Raids. (National Bureau of Standards Handbook H 38, May 1942.) U.S. DEPARTMENT OF COMMERCE, Washington, D.C. 10 cents.

Optical and Mechanical Characteristics of 16-millimeter Motion-Picture Projectors, by R. E. STEPHENS. (National Bureau of Standards Circular C 437, June 1942.) Pp. 22. U.S. DEPARTMENT OF COMMERCE, Washington, D.C. 10 cents.

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